

Computers, Conservatives and Governments

SIR John Eden seems to have done a great deal to make the British Government's attitude towards industry at large applicable to the British computer industry (see page 4), for it is only reasonable that a government which could let Rolls-Royce go bankrupt should take away from the largest computer manufacturer the sense of sponsored security which it has enjoyed since its formation. The principle on which the government appears to be working is that expressed somewhat over-directly by Mr John Davies, the Minister for Trade and Industry, some months ago, that industry should no longer expect the government to bail it out. What Mr Davies and his colleagues intend is that industrial organizations of all kinds should be much more aware than in the past that the penalty of failure is not humiliation or even a government subsidy but the actual disappearance of a commercial organization, factories, jobs and even boards of directors.

Up to a point, it will be valuable for everybody in Britain (as well as for customers of British companies abroad) if the sense of urgency which the government is seeking to impart to industry does actually lead to a more competitive and more hardnosed view in British companies about the objectives of all commercial enterprises. There are, however, several pitfalls in a simple application of this doctrine, not the least of which is that the present government could easily be a long way towards a demonstration that the government's word is not to be taken over-seriously. After all, ICL is itself a child of what seemed at the time a shotgun marriage of computer manufacturers organized by the old Industrial Reorganization Corporation. Whether or not the government formally agreed to keep ICL's interests close to its heart, there is very little doubt that the merger would never have come to pass if an impression of security and even subsidy had not been created. In many ways, the case of ICL resembles very closely that of Rolls-Royce Limited, where it is unlikely that a consortium of banks would last year have helped to tide the company over its shortage of cash if there had not been a widespread belief that no government could let that jumped-up motor manufacturer go to the wall. One of the questions which has been raised by the Rolls-Royce decision and that on ICL is, in short, whether industrial companies can allow themselves to rely for long-term planning on the implicit promises of governments which must necessarily be comparatively short-lived. Another way of putting the same question is to ask whether governments should not somehow be constrained in their relationships with industrial companies by the need to be committed by their predecessors' implicit promises. That is a serious question which needs more careful discussion than it has received so far.

For the rest, the new government's decision that in future ICL should stand on its own feet raises important questions about the management of such a company based in Britain but seeking somehow to compete with the world at large. The problem is different from that which faced Rolls-Royce, for there is no question of being committed

to an over-grandiose project that dwarfs the rest of the company's business. By the same test, ICL is not in the position of the aircraft manufacturers busy in the development of the Concorde aircraft, by now persuaded that their commercial success depends entirely on the success of the machine they are trying to construct but uneasily aware that it is becoming the tail that wags the dog. What ICL is trying to do is to be all things to all customers—a supplier of very small special purpose computers and very large general purpose machines. Its handicap is that its reputation in the field is not nearly strong enough for it to be able to sell successfully without trimming its prices to get the business, which means that a disproportionately small part of its resources can be spared for the kind of research and development on which the company's success must in the long run depend. The trouble, alas, is that no amount of native wit nor even of ingenuity can be certain to overcome this fierce obstacle. As always since the previous government set about creating the general purpose computer company, there is now a danger that ICL will be squeezed between the very large international manufacturers such as IBM and the comparatively very small companies which specialize in the manufacture of computers for particular purposes. Its continued existence is in short a reminder of the inconsistencies or at least the imperfections of the Labour Government's doctrine that size means strength.

The most obvious difficulty is that diversity is itself a weakness if not supported by sufficiently diverse research and development. This is one sense in which high technology differs from low technology. For ICL, the strategy now should be to attempt to concentrate its resources on some field in which it can hope to make a sure footing for itself. On the face of things, the most likely place in which to dig in the heels will be the design and manufacture of very large computers, for here at least is a set of circumstances in which the government is certain in the next few years to be such a ready customer that no amount of self-denial can put ICL entirely out of court. For the government, the moral of the situation which has now arisen is that it must somehow in the next few years create a climate in which comparatively small computer manufacturers can make a way for themselves in a competitive world by offering other kinds of special services. All kinds of possibilities suggest themselves. It is hard, for example, to believe that it must indefinitely be necessary to rely on general purpose machines for such functions as the control of automatic machine tools or typesetting equipment. And what of the chances that specialized kinds of computers will in due course be devised for satisfying the needs of particular kinds of customers? And is there not a case for thinking that the computer manufacturer who first manages to market his machinery in such a way as to satisfy not merely the ego but the need of his customer will have a powerful competitive edge? The modern world, after all, is almost as full of disappointed computer owners as of computers.



But how can an ostentatiously *laissez faire* government hope to influence the direction of technical development? This is a question for Mr Davies as well as for the Chancellor of the Exchequer, Mr Anthony Barber. It has in the past few months been too easily assumed that there is nothing that can be done without compromising principles. After all, even the Conservative government has kept in being the very large apparatus for the conduct of research and development in the civil fields. Even after months of declarations that people must now stand on their own feet, all kinds of research laboratories that might have been abolished find themselves still in existence. Would it not make sense to ask that some of these should more obviously than in the past lend their energy to the development or at least the support of new technical developments in computers? Indeed, there are the strongest of reasons for asking that the government should

devise some way of using the apparatus of public research and development for encouraging technical development of a kind that seems to be desirable. In the United States, where public laboratories are far thinner on the ground, the Federal government undertakes the corresponding responsibility of letting research and development contracts with industrial companies. One of the paradoxes of the new Conservative government's philosophy is that it has kept much of the apparatus of public intervention in industry which the Labour government set up or inherited, but has resolutely decided not to use it, and has at the same time foresworn the devices by means of which other kinds of governments seek to use industry for the creation of national prosperity. In short, the new Conservative government is a little like a boxer who decides not to use one fist and who ties the other behind his back.

How will the Universities be Nourished?

THESE are bad times for British universities, as any academic can tell by reading between the lines of the latest report from the University Grants Committee (see page 5). What most of all afflicts the committee is the uncertainty about the future which has been occasioned partly by the way in which the Department of Education and Science has been muttering at the high cost of present arrangements, partly because of the difficulty of predicting what students will ask of the university system, partly because of uncertainty about the relationships between the universities and other sectors in the British system of higher education such as the polytechnics and partly because, in any case, the committee is already deeply immersed in planning for the next quinquennium, due to start in September 1972, and because it is, as always, uncertain of the outcome. In circumstances like these, it is perhaps too much to ask that academic planning should be a tranquil business, yet there is enough evidence in the recent trend of events to show that some at least of the present uncertainty could be removed.

Where the government is concerned, the most urgent need is for a declaration of policy. To say, as the government does, that higher education is a good thing and that there should be more of it begs questions such as the extent to which institutions of all kinds, from colleges of education to universities, can rely on finding money in the years ahead. In the past few years, the universities have been compelled by successive waves of economy and increases of costs to regard themselves as comparatively poor relations of the polytechnics, until now almost universally the apples of the local council's eyes. But nobody can be sure that current pressure on local authority finance will enable the polytechnics to continue living as freely as they have in the recent past. Indeed, there is a danger that when the local authorities turn mean, they will be able to do so individually and privately, thus turning prosperity into penury without the outside world knowing what is afoot. Then, to be sure, it may be much easier than at present for the polytechnics to swallow their recent pride and make common cause with the universities. The danger is that by that time, the universities themselves

will be so overwhelmed with immediate problems that they will be unable to take part in the marriage which they now profess they seek. In circumstances like these, the issues of how to find a uniform framework within which to finance all parts of the higher educational system, the colleges of education as well as the polytechnics and the universities, and a common system for selecting students, should not be ignored simply because they are uncomfortable.

For the universities, there is an urgent need for better arrangements for coordinating academic enterprises. In the past few years, the University Grants Committee has not been nearly as successful as it might have been in bringing about a more rational pattern of teaching. Although it is clear that there are far more universities per square mile in Britain than are necessary to enable students to sit at the feet of some teacher or other, the universities have been uncommonly slow to arrive at a sensible division of labour among themselves. If one university sets up a chair of applied Sanskrit, for example, there is still a danger that others nearby will try to follow suit.

The University Grants Committee has in the past made a few efforts to rationalize the pattern of teaching in, for example, agriculture, but has in the process burnt its fingers so badly that it has fought shy of other ventures. Yet there are good reasons to expect that rearrangements of this kind could bring great economies and, by extension, greater freedom. As things are, it seems to have been left to the research councils to create a tendency towards specialization among the universities. In the long run, no doubt, the result will be as beneficent as the largely enlightened management of the research councils can make it, but this unfortunately is only half a reassurance. It would be better and safer if the universities as such were more directly in charge of their own destiny. Everybody will acknowledge that present uncertainties about government policy must make the University Grants Committee unsure of itself, but there is no excuse for not carrying out good housekeeping while waiting to see which way the wind will blow.

Who will Referee the Referees?

ONE of the most contentious issues in modern science is the institution of the referee—the system by means of which anonymous, devoted and skilled professionals spend their energy on the evaluation of scientific articles submitted for publication. Journals such as *Nature* would not flourish without the referee. But if in some quarters referees are highly respected, elsewhere they are often reviled. Disappointed authors fight long wordy battles with their unknown critics and, for every man who says that he has valued what the referee has had to say, there is at least one other who bristles fiercely at what he supposes to be an attack on his professional integrity. In circumstances like these, anything that throws light on the way in which the referee system functions is likely to be valuable. An article by Dr Harriet Zuckerman and Professor Robert K. Merton in the current issue of *Minerva* (9, 66; 1971) is to be welcomed even though in many ways the document misses the chief question to be decided—what are referees for?

Much of what Zuckerman and Merton have to say is based on a study of the complex of journals published by the American Institute of Physics and especially *Physical Review*, the house journal of the American Physical Society. One of the merits of the article, however, is the reminder which it provides that—contrary to expectation—scientific journals are less choosy about the articles they publish than journals in history, philosophy and the like. Although there is no doubt fierce competition for space in journals such as *Physical Review Letters*, the top of the pecking order of rejection rates among learned journals seems to be occupied by those that specialize in history: of three journals examined in 1967, the average rejection rate is 90 per cent. By comparison, even physics comes a long way down, where twelve journals were discovered to have a rejection rate of 24 per cent. One of the weaknesses of the argument which Zuckerman and Merton put forward shines out—if that is the phrase—from their discussion of this phenomenon. Is a journal which publishes only a tiny proportion of the manuscripts it receives to be praised for the toughness of its editorial policy or despised for rendering such a poor service to its contributors? The trap in all this is to suppose that there is some necessary correlation between an editorial decision to publish an article and the quality of what is published. The truth is, or should be, that journals should have a character of their own which is at least recognizable to those in charge. Just as a journal for the popularization of science will sometimes decline to publish articles which are too didactic or too conceptually difficult for its purposes, and as a daily newspaper will decline to publish articles which have very little to do with current events, so scientific journals with a place to keep will decline to publish articles that do not suit them. To say that a decision not to publish is necessarily a comment on the quality or the integrity of a putative article is to make too much of a simple and valuable property of all kinds of publications—that they must seek to be not so much arbiters of the competence of their contributors as servants of the interests of their readers.

What Zuckerman and Merton have to add to this discussion is an intriguing analysis of the relative status of those who referee and those who are refereed upon. Their

sample consists of those contributors to *Physical Review* submitting manuscripts between 1948 and 1956—close on 10,000 altogether. By hunting through the reference books, Zuckerman and Merton have been able to establish the pecking order within this class of physicists—Nobel prizewinners at the top and humble authors at the bottom. Using the same criteria, referees are also divided into categories at different places in the pecking order. Every disappointed author's prejudices will be confirmed by the discovery that the manuscripts of higher ranking authors are dealt with more quickly than those of physicists at the bottom of the pecking order—in the period studied by Zuckerman and Merton, 42 per cent of those manuscripts in the top drawer were dealt with in less than two months but only 29 per cent of what are called third-rank physicists enjoyed this speed. By the same test, referees appear to have been more tolerant of the manuscripts submitted by higher ranking physicists than of those coming lower down, although there is at least one sign of grace, that there seems to be no correlation between the age of an author and the acceptability of his manuscript or between the rank of a referee and the nature of his decision. The question which neither Zuckerman and Merton nor their readers will be able to answer is whether referees can ever be the Solomons authors, referees and sometimes even editors suppose that they must be.

100 Years Ago



The Duke of Edinburgh in Ceylon: a book of Elephant and Elk Sport. By John Capper, *Times* correspondent. Illustrated with chromo-lithographs. (London: Probst & Co., 1871.)

THIS book is sufficiently described by its title, being a record of the visit of the Duke of Edinburgh to Ceylon last year, and of his success in the colonial sports of elephant hunting and elk hunting. It appeals to two sections of the public, those who eagerly seize upon every incident connected with the mode of life of any member of our Royal family, and those who are equally eager after any description from life of sport in those countries where wild beasts worthy of a hunter's rifle abound. We may quote the following as an instance of the perils encountered by our Prince in navigating the Cingalese rivers. "The stream was teeming with life. Fish of all varieties and sizes sprang into the boats as they paddled along, one of them finding its way into the Prince's coat pocket" (loyal fish!); "on all sides could be heard the snapping of alligators' jaws as tiny fish were caught in the monsters' mouths. The party had proceeded about a mile down the stream, when one of them, leaning down and resting his head on the gunwale of the boat, was startled from his quiet rest by the apparition of an alligator's gaping jaws, which made a direct snap at his head, fortunately missing it, but seizing, in place of it, the barrel of the rifle held in the hands of the Prince's English attendant, who was seated next to him, and which the monster nearly wrenched out of his hand, splashing the water about, and drenching every one in the canoe." Is the *Times* correspondent quite certain that alligators are found in Ceylon?

From *Nature*, 3, 366, March 9, 1871.

OLD WORLD

Select Committee Computes ICL's Future

WHAT would be the consequences if International Computers Limited were to follow Rolls-Royce down the slippery slope? That gloomy prospect was the thrust of much of the questioning at last week's meeting of the computer subcommittee of the Select Committee on Science and Technology in the House of Commons. Giving evidence was Sir John Eden, the minister at the Department of Trade and Industry in whose ambit lie the affairs of the computer industry. Sir John obviously thought the questions to be out of turn. ICL, he said, is perfectly capable of standing on its own feet. Following the merger in 1968 when ICL was formed out of ICT, English Electric Computers, and the computing interests of Plessey, the company has built up a strong management team and a clear idea of the market. The British computer industry is in no need of propping up.

In this rosy light, Sir John revealed two new aspects of the government's policy toward the industry. Both affect ICL, but in Sir John's view in neither case significantly. First, the practice of loading the selection procedure for government computers in favour of ICL will end. This measure should close up some old wounds, chiefly among the United States companies which have factories in Britain. Honeywell and IBM claim to be good guests, contributing favourably to the balance of payments, but are discriminated against in orders from the British government.

Second—but probably less important in practice—the government seems now to consider that in exceptional circumstances it would feel free to buy its very large computers from companies other than ICL.

Adding these two principles to the news a fortnight ago that the government intends to rely less on its own resources to write its own computer programs (see *Nature*, 229, 588; 1971), a picture of the attitude of the Conservative government to the industry is now rapidly emerging. All aspects of the new policy have so far been broadcast through the subcommittee, which must be gratifying to its chairman, Mr Airey Neave. Since the computer investigation got under way more than a year ago, the subcommittee has earned a reputation for directing searchlights into awkward crannies, just as its twin subcommittee on space research is at present exposing the absence of thought behind British space policy.

So far, however, the Conservative government has not followed up the reorganization of the old Ministry of Technology with a revision of the obscurities which confuse the government's arrange-

ments for dealing with the computer industry. One of the successes of the subcommittee last year was to point out the deficiencies of a system involving the Ministry of Technology, the Civil Service Department, the Treasury, the Stationery Office, a group called the Technical Support Unit shared between the Ministry of Technology and the Civil Service Department, and a network of inter-ministerial committees. When a government department decides that it needs a computer, the requirement is first discussed between the particular department and the Civil Service Department. The role of the Ministry of Technology was to draw attention to the computers available from firms manufacturing in Britain, and the Treasury is involved to make sure that the user department gets value for money. Finally, the order is placed through the Stationery Office.

Briefly, the role of the Ministry of Technology was to stimulate the use of computers, encourage computer firms to



Sir John Eden.

set up factories in Britain, and generally to sponsor the industry. The only change since these functions were taken over by the Department of Trade and Industry, so far discerned by the subcommittee, is that individual government departments are becoming more competent at handling their own computer projects (according to the most recent evidence from the Civil Service Department). The Civil Service Department is therefore retiring from the scene where it can, to search for a more strategic role in planning, the proposal of common policies, and the promotion of technical developments.

But although nobody has poured weed-killer on what Mr Neave used to call the data-processing jungle, attitudes towards the computer industry have changed. Software firms have often asked that Whitehall emulate the United States government which carries out hardly any software work internally. Even defence software, where it might be thought that

secrecy would be a problem, has been contracted out. Part of the justification for the new policy, which will result in an increase in the volume of business placed with software houses from £570,000 in 1969/70 to possibly more than £1.5 million in 1971/72, is the high rate of turnover in government data-processing staff, although recent figures show a slight improvement. For example, 11.8 per cent of the number of experimental officers in data-processing at April 1968 had left a year later, but in the twelve months of 1969/70 resignations were only 9 per cent of the experimental officer staff at April 1969. Whether these figures are any worse than the commercial software firms could show is a moot point.

Perhaps this new policy will lead to a consolidation of British software firms, most of which are tiny by American standards, although this is something that the Industrial Reorganization Corporation failed to achieve in its brief life. More to the immediate point, it is to be hoped that the new approach to software will mean an end to the mentality that, according to evidence given to the subcommittee last year, allowed twenty-three payroll systems for use on ICL 1900 machines in government departments to be developed separately.

At least one idiocy has been done away with by Sir John Eden's new policy—the riddle when is a British computer not a British computer will no longer have to be answered. In the past, when competitive orders were sought from several firms, the contract has been awarded on merit but with a preference for the British machine so long as it was not unduly expensive compared with the others. What the price differential can rise to has never been officially stated. But the managing director of IBM (UK), Mr Edwin Nixon, caused a furore last year when he claimed that non-British companies have to tender 25 per cent lower than ICL to get government orders. The situation is undoubtedly more complicated than that. But apart from the ill feeling caused by a policy that has never been fully defined in public, the system has run into problems with computers manufactured in Britain with a significant proportion of British-made components, but by a non-British company (notably IBM and Honeywell which both have factories in Britain). The Department of Trade and Industry has already admitted to the subcommittee that it is sometimes necessary to examine each of a firm's models individually and in detail to decide to what extent it can be called British.

What has the change of government meant for ICL? Sir John Eden affirms

that the Conservative government attaches great importance to the existence of a strong, competent, British-owned computer industry. Britain must continue to be able to make a full range of computers. The new policies will not adversely affect ICL, Sir John claims, now that the British computer industry is capable of meeting and beating competition on its own terms. In any case, he points out, the government sector takes less than 10 per cent of ICL's sales. The chairman of ICL, Sir John Wall, is reported as commenting that ICL has never won a contract through the price preference arrangement. In that case little has been gained by the old policy, at the cost of a deal of discontent.

In the opinion of Sir John Eden, the mode of help for ICL in future will be the government's procurement policy, coupled with an intensification of the use of computers in the public sector. With only £38.8 million worth of computers in operation in government departments at the end of last year, there seems to be room for growth. And the government will continue to encourage buyers in the public sector to favour British machines.

All the same, members of the subcommittee were far from convinced by the optimism. The government has already made clear that there will be no more payments to ICL after the final £2.25 million due under the merger agreement is paid this year. And the withdrawal of investment grants which have been particularly favourable to the computer industry is causing concern among the committee. Would the government step in if there was an acute cash shortage in the industry, members of the subcommittee wanted to know? Sir John was uneasy about the question. It is the policy of the Conservative government to encourage industry to rely on its own sources of finance without going to public sources, so it was unlikely that the government would go beyond the role of purchaser of computers. But faced with a hypothetical question, nothing could be ruled out.

The subcommittee was pleased that Sir John was sympathetic to the notion of development grants, however, particularly in regard to Project 52. This is the code-name for ICL work on a new large machine, still under wraps. But the subcommittee was clearly worried about the future of Project 52 now that ICL has joined with the American Control Data Corporation (CDC) and the French Compagnie Internationale pour l'Informatique (CII) to form an organization called Multinational Data that will integrate the range of hardware offered by the three companies. Control Data dominates the market for the largest computers, an area in which even IBM has never succeeded in entering convincingly; does the arrangement mean that ICL will be constrained to specialize in medium-sized machines? Sir John did not think so.

If advantage is taken of the link with

CDC, however, it could be ICL's salvation for the next decade, by providing a foot in the door of the American market. Perhaps it will also solve the problem of the British inability to match the American R&D expenditure on computers. So far the subcommittee has not been able to discover precisely how much is spent annually under this heading in Britain. Roughly speaking, it seems that the British computer industry might be spending about £20-£25 million a year, and the government (through the Department of Trade and Industry) about £16-£17 million. But it was astonishing that the representatives of the Department of Trade and Industry who have appeared so far have not been able to put their fingers on precisely how much the government is spending and under what headings. To the subcommittee this must be evidence that not enough attention is being paid to the basic problem—ICL barely has the resources to maintain an R&D programme that will sustain the company in the years ahead. The 360 series is reputed to have cost \$3-4 thousand million to develop, and its introduction was traumatic even for a company the size of IBM. How can ICL match that?

UNIVERSITIES

UGC in the Dark

THE University Grants Committee has evidently found itself in a cleft stick during the past year. It has had to take decisions about capital expenditure on university buildings up to 1974, but the framework for these decisions has been missing because there has been no indication from the government of the size to which the student population in the universities should grow. Indeed, it seems unlikely that the government will be in a position to make up its mind about the rate of growth in higher education until the middle of 1972, when decisions on recurrent expenditure during the next quinquennium (1972-77) must be announced. The committee in May last year therefore took the law into its own hands, and sent each university a letter indicating the student numbers in 1976-77 that the university might consider as a starting point for its own planning. Mr Kenneth Berrill, chairman of the UGC, outlines in the UGC's annual survey the likely drift of planning in the universities in the light of replies to this letter (*University Grants Committee Annual Survey, 1969-70*, HMSO, 30p).

The shortage of residential accommodation for students is likely to be the chief constraint on expansion unless loan-financed housing schemes can be operated on a more effective basis. The UGC has already been authorized to increase its assistance to loan-financed schemes, and instead of only providing

furniture grants, as it has in the past, the committee can now contribute to the overall cost of new buildings. Nevertheless, many universities are uneasy about raising money from private loans, and Mr Berrill says that the whole question needs further study.

Leaving aside limitations imposed by the shortage of residential accommodation, the universities indicated that they could collectively accommodate 285,000 students by 1973-74, 41 per cent of whom would be arts based and 59 per cent science based. But this imposes another constraint on the UGC's planning because it would like to see a mix of 45 per cent arts students and 55 per cent science students—much the same as that which obtains among students at present entering universities. The UGC has therefore found it "necessary to restrict to the minimum the provision of extra accommodation for science subjects, and to give heavy emphasis to the provision of additional capacity in the arts and social sciences".

The UGC's survey also shows that the cost of educating a student at a British university increased from £752.5 a year in 1959-60 to £900 a year in 1966-67. By 1968-69, however, this cost had dropped to £864.3. The staff:student ratio also fluctuated during the 1960s, falling from 8.04:1 in 1959-60 to 7.55:1 in 1964-65, since when it has been increasing steadily to reach 8.16:1 in 1968-69. What will happen to these figures in the next ten years is a moot point, however, because, as the UGC makes clear, "the final target for 1976-77 can be determined only in the light of government decisions on capital and recurrent finance" and this in turn calls for government decisions on the size and structure of higher education.

CANCER THERAPY

Issels' Claims Refuted

MANY of the patients whom Dr Issels claims to have treated successfully in his cancer clinic at Ringberg, Bavaria, were cured by radiotherapy or surgery before they were admitted. Confusion about efficacy of the treatment has therefore arisen because severe post-radiation reactions were misdiagnosed as residual cancer and treated as such by Dr Issels. That is the chief conclusion arrived at by a team of British cancer workers who visited the clinic in January at the request of the Joint Co-ordinating Committee on Cancer Research. Reporting on its findings, the team is lavish in its praise of the standards of nursing given to patients in the clinic, but sceptical of the claims advanced by Dr Issels for his treatment (*A Report on the Treatment of Cancer at the Ringberg Clinic, Rottach Egern, Bavaria*, HMSO, 15p). The members

of the team say "we are convinced that Dr Issels believes implicitly in the treatment he gives. We think he does a great deal to help most of his patients. We sadly think, however, that he is misguided in his beliefs, and that the treatment peculiar to his clinic is ineffective".

The committee's conclusions are based chiefly on a study of the records of 48 patients treated in the clinic for various types of cancer, and who had survived or were thought to be free from disease between two and twenty-one years later. As far as Dr Issels was concerned, all these patients had terminal cancer when they were admitted to the clinic, but according to the committee, 28 of them showed no evidence of residual tumour—their symptoms were more probably due to over-radiation—six were treated with cytotoxic drugs, two had tumours removed surgically after treatment by Dr Issels and one probably never had cancer.

In only eleven patients was there "possibly some evidence of tumour regression" after treatment by Dr Issels' method on its own. But in the committee's opinion, eight of these possibilities rested on such slender evidence that "only by giving the benefit of much doubt could they be included", and the remaining three provide almost the entire evidence that a favourable tumour response results from the clinic's treatment when used alone. Of these three, however, one had no histological confirmation of the diagnosis, one was of low-grade malignancy and the other was benign or at most of marginal malignancy. The committee therefore concludes that "they might perhaps have been accepted in some hospitals as successful results of a treatment method under trial, and we do not believe they would have been so accepted in most; they would not have been so accepted in the hospitals in which we work".

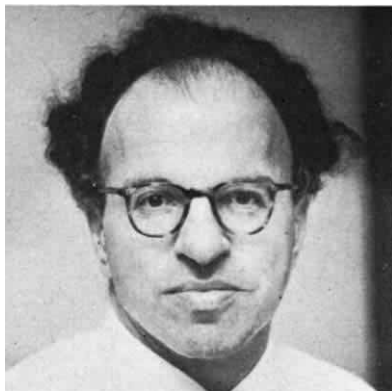
But while the committee pours considerable scepticism on Dr Issels' claims to cure terminal cancer patients, it is no less vociferous in its praise of his nursing care. The strength of Dr Issels' character and the frankness with which he treats his patients often give them renewed hope so that they are "mentally and morally renewed" and severe pain is sometimes relieved without drugs. The committee ends with a gloomy reflexion on the standards of treatment for terminal cancer patients in Britain; "if regular and continued support was always available for the dying, there would be less inclination for patients to seek help abroad. . . . The fact that so many patients go to this clinic to find something they fail to receive at home is really more a reflexion on the medical service they leave than a credit to Dr Issels' particular treatment". It is also a reflexion of

the power of the mass media in stirring up public feeling over a sensitive issue such as cancer therapy.

ESRO

New Man at the Helm

A GLIMMER of hope that the European Space Research Organization may be emerging from the doldrums was provided by the election last week of Dr Alexander Hocker to the post of Director-General of the organization. Dr Hocker will take over from Professor Hermann Bondi who became scientific adviser to the Ministry of Defence in Britain on March 1. Professor Bondi himself was known to be sceptical of the possibility that the ESRO council would find a suitable director to take over an organization whose future hangs in the balance. But on February 23, the council had little difficulty in electing Dr Hocker to the post, and he will take up his duties on April 15.



Professor Hermann Bondi.

A former member of the Federal Ministry of Atomic Energy in Germany and of the council of the nuclear research centre at Julich, Dr Hocker played an active role in the formation of ESRO and was chairman of the ESRO council from 1964-67. He is at present a scientific adviser to the board of directors of Volkswagen.

Dr Hocker has a formidable task ahead of him in steering ESRO through its present crisis. At the council meeting last December, the French delegation gave notice that the French government had decided to withdraw from ESRO at the end of 1971. This declaration seemed to throw the other delegations into some confusion, although the French government later admitted that the declaration was a bargaining ploy, and that a final decision on secession will be taken later. Nevertheless, the council meeting passed a resolution which would enable other countries to withdraw at the same time as France, provided they announce such an intention by the end of June 1971. At the first council meeting last December, the French delega-

tion refused to pledge full support for the programme of applications satellites, and this is one of the chief stumbling blocks that Dr Hocker must remove if he is to ensure that ESRO has a viable future.

CORROSION

The Price of Neglect

A NATIONAL Corrosion and Protection Centre should be set up to act as a focal point for all the knowledge and expertise at present spread rather unevenly throughout Britain. That is one of the chief recommendations set out in the report of the Committee on Corrosion and Protection (HMSO, 1971; 80p), and the committee adds that such a centre could be situated near an existing scientific establishment to make use of such facilities as a library and computer.

Set up in 1969 by Mr Anthony Wedgwood-Benn to survey all aspects of metallic corrosion and its prevention, the committee, under the chairmanship of Dr T. P. Hoar of the University of Cambridge, has produced a stark analysis of the total cost of corrosion and protection in the UK. This amounts to about £1,360 million per year. A close investigation of individual industries has revealed that about £310 million could be saved each year if more attention were paid to protection from corrosion—the total cost of corrosion is about 3.5 per cent of the gross national product in the UK compared with about 1.25 per cent in the US, for example.

Most of the saving could be made by increased awareness and improved information services on a national scale, the committee suggests, although the increase of knowledge about corrosion is obviously important. The cuts in corrosion costs which the committee thinks are possible in different industries vary widely from about 8 per cent for the oil and chemical industry to more than 40 per cent in the power industry. The very high value for the power industry reflects, in the committee's opinion, reluctance to make extensive use of cathodic protection methods for piping, and a worrying lack of knowledge at the design stage of equipment operating conditions. Government departments do not escape lightly and the armed services are criticized for lack of corrosion awareness among designers. The army owns many pieces of equipment, for example, which are stored throughout much of their life but must be ready for instant use and so have to be replaced from time to time because of corrosion.

Appropriate action at the design stage is the rule only in the aircraft and oil and chemical industries but in other sectors "attention to corrosion is largely confined to avoidance of bad

mistakes made in the previous designs. This method of learning by trial and error is expensive and wasteful". The report also contains an assessment of the extent to which corrosion and protection are dealt with in educational courses of all kinds and comes to the conclusion that the training of engineers, designers and architects is generally deficient in anti-corrosion skills.

NUCLEAR ENERGY

Oil or Nuclear Power?

THE decision by the Central Electricity Generating Board to defer construction of the 2500 MW Sizewell B nuclear power station until at least 1972 and to go ahead during 1971 with an oil-fired station on the Isle of Grain flies in the face of those who were predicting that the recent oil price rises would swing the balance of argument firmly in favour of nuclear power stations, in spite of their higher capital cost. The construction costs of these two power stations are now estimated to be about £250 million and £200 million respectively, even though the capacity of the Isle of Grain station is about 25 per cent greater than Sizewell B.

The CEGB has based its decision ostensibly on the smaller annual rate of growth of demand for electricity which the Electricity Council now predicts for the next five years (5 per cent rather than 6 per cent). The peak demand of 54,000 MW which was anticipated for the winter of 1975 to 1976 is not now expected to be reached until the following year, and the board therefore argues that it is not necessary to start work on Sizewell B for at least a year. But it seems that the CEGB does not feel by any means committed to the Sizewell project even in 1972, in spite of an investment of more than £1 million in the preparation of the site. The board may see the trimming of its nuclear power programme as a way of meeting the cuts required in capital spending which will soon be spelt out by the Treasury. The fact that the capital cost accounts for about two-thirds of the total cost over an average lifetime for a nuclear station must certainly weigh heavily with the board in the prevailing economic climate; the proportion is nearer one-third for an oil-fired station.

The Sizewell B design contract was awarded to the Nuclear Power Group (NPG) last November and the decision to mark time on the project has sent quite a chilly wind through the whole British nuclear power industry. There seems little immediate danger of the NPG running short of work, however, because it is involved in the construc-

tion of two other nuclear power stations, Hinkley Point B and Hunters-ton B; these are both 1250 MW stations powered by advanced gas-cooled reactors—the same type as planned for Sizewell B. The NPG has also been careful not to rely entirely on nuclear power contracts and has, for example, the design contract for a 2000 MW oil-fired station at Inverkip on the Clyde.

COUNTRYSIDE

Power at the Top

THERE was evident satisfaction at the Countryside Commission when the chairman, Mr J. Cripps, announced on March 1 that the government had agreed to let the commission have a director at under-secretary level in the Civil Service. The change is more significant than it may seem. Mr Cripps was confident that an under-secretary will have more influence with ministers than the assistant secretary who has been the chief officer of the commission since its inception in 1968.

There can be no doubt that great things are expected of the new director, who is likely to be appointed in the next few weeks. The recent concession also includes a relaxation of Treasury control on expenditure. But the commission still has plenty of problems obstructing the full implementation of its job as the statutory national body responsible for matters relating to the conservation and enhancement of landscape beauty and amenity, and to the provision of facilities for the enjoyment of the countryside.

The greatest curb to the commission's activities is its lack of staff, and there seem to be no signs that the government will allow any increase in the near future. For this reason it is hoped that the officers of the commission will be consulted earlier in regional planning schemes so that some costly public enquiries can be avoided. Another grievance with the government is its decision to move the commission out of London. The commission does not want to go, saying that most of its business is transacted in London. The struggle will doubtless continue; meanwhile a nine months extension has been secured on the lease of the commission's headquarters at Cambridge Gate.

Parliament in Britain

Scientists' Unemployment

MR DUDLEY SMITH, Under-Secretary of State, Department of Employment, said that on December 2, 1970, there were 2,983 unemployed persons on the technical and scientific section of the professional and executive register. This figure includes 175 women. The information was given in reply to a question from Mr R. Carter. (Written answers, February 25.)

Abortion Act

A COMMITTEE of enquiry has been set up to review the workings of the 1967 Abortion Act. The enquiry will be concerned with the way the act is working and not with the principles that underlie it, and the chief conditions for legal abortion will remain unaltered. Announcing this in a statement given to the House of Commons, Sir Keith Joseph, Secretary of State for Social Services, also said that the government proposes to encourage the growth of local authority family planning services, especially in priority areas. Provision has therefore been made for expenditure on this service to treble by 1972-73.

Mrs Justice Lane will be chairman of the committee, and other members will be appointed later. The terms of reference of the committee are "To review the operation of the Abortion Act, 1967, and, on the basis that the conditions for legal abortion remain unaltered, to make recommendations." (Statement, February 23.)

Desalination

THE government is not yet ready to make a decision on whether to support construction of a pilot plant, situated at Ipswich, for production of desalinated water. Lord Sandford, Parliamentary Under-Secretary of State, Department of the Environment, said that the Secretary of State for the Environment should soon be able to come to a decision about the project. It would cost about £1.5 million. But Lord Kennett pointed out that when he was sitting in Lord Sandford's place nine months ago, before the election, he announced that the government was in the last stages of its consideration of the project. He therefore wondered why there has been such a delay in coming to a final decision. Lord Sandford replied that the government has been reviewing several schemes that were given the go-ahead by the previous administration, and he reaffirmed that a decision will not be long delayed. Lord Nugent of Guildford said that after a gestation period of nine months, there should be a reasonable prospect of providing an answer. (Oral answers, Lords, February 24.)

NEW WORLD

Sweetness and Light Break Out Again

by our Washington Correspondent

THE science policy making machinery in Washington is now running in perfect order, and the disharmony between its working parts that caused such discordances last year is already a distant memory, incapable of recurrence. This at least was the impression effortlessly given last week by the leaders of the various science policy making sections of government, between whom almost no trace of disagreement on any matter of substance was allowed to emerge. Among the auguries of this harmony is that the semi-increased budget of the National Science Foundation is assured of a reasonably comfortable passage through Congress. The only discordant note in the orchestra was the strident pitch made by the President's science adviser, Dr Edward E. David, on behalf of the SST.

David and other luminaries of government science officialdom were speaking at a seminar on science and public policy organized by the Council for the Advancement of Science Writing and held at the National Academy of Sciences from February 22 to 24. By far the most important utterances in the seminar were those of Congressman Edward P. Boland, making his first public appearance before the scientific community, over whose affairs the iron vagaries of the Congressional seniority system have now given him as powerful an influence as anyone in Washington.

As new chairman of the House appropriations committee that fixes the budget of NASA and the National Science Foundation, Boland will have a greater say in how these agencies operate than any members of the House Science and Astronautics committee or its subcommittee on Science, Research and Development. The latter committee has gained more public attention, particularly because of the hearings on many aspects of science policy conducted under its former chairman Mr Emilio Q. Daddario, but as an authorization committee its tangible effect on the funding of science is limited to setting an upper limit on the sum the house appropriations committee may appropriate for the NSF and NASA. Since the authorization committee seldom allows less for its agencies than the appropriations committee wants to give, the whip hand lies with the latter, and the only obvious restraint on an appropriations com-

mittee's will is that it must reconcile its differences with the counterpart committee of the Senate.

Boland's opinions on science and public policy, therefore, were listened to attentively, and although he gave no sign of the partisan fervour for science that everyone would like to see in an appropriations committee chairman, his attitude seemed at worst to be that of friendly neutrality. He complimented the director of the National Science Foundation, Dr William D. McElroy, calling him a "persuasive and diplomatic personality", and went on to observe that the NSF usually "gets about what it wants" in the appropriations process. McElroy will be particularly pleased if this reflexion turns out to apply to the NSF's 1972 budget under Boland's chairmanship since with a budget authority of \$622 million (\$116 million more than last year) the foundation has more than ever to lose at the trimmer's knife.

Although the NSF is only a small part of Boland's concern—his subcommittee also decides appropriations for the Department of Housing and Urban Development, NASA and a rag-bag of smaller government offices—he has been a member of the subcommittee for some 16 years and appears well acquainted with the NSF's problems. He laughingly remembered the day when he was assured the foundation would never require more than \$20 million a year for its support, but quickly set everyone's minds at rest by voicing his judgment that "the budget of the NSF will increase substantially in the years to come", chiefly to pick up the research being abandoned by other agencies.

Boland believes that responsibility for basic research is tending to become concentrated in the NSF although he sees serious dangers in the amendment tacked on to the military procurement bill by Senator Mansfield two years ago that set this trend in motion. "It seems to me that over the years the great breakthroughs we have had have come about because there has been basic research carried out by many agencies and many people. There may be duplication but this is not harmful. I think that when you have many agencies doing basic research there are more opportunities to acquire knowledge", Boland declared.

Boland's indications that the NSF would not get roughed up in Congress

were followed by equally comforting assurances that science's two protectors in the executive are now at loggerheads no more. Mr William D. Carey, assistant director of the Bureau of the Budget (now the Office of Management and Budget) until 1969, told the seminar last week that there exists "a strong built-in disposition in the Office of Management and Budget for the support of science and technology. If the OMB could do what it wanted the hunger pains in the scientific community would never have been felt". The implication of Carey's remarks is that science might have retained its 15 per cent growth rate throughout the Johnson Administration were it not for another important factor in science funding, "the extent to which a sitting President has a glandular reaction to science and scientists" as Carey put it.

The honeymoon's ending was also soured by the attitude of the scientific community that all the favours given to them were no more than they deserved on moral grounds. "Don't blame the government for letting science down", Carey said. "The trouble was that scientists wanted to be rocked in a cradle, they didn't believe that summer was gone and winter coming". The chilliest part of the winter that ensued was when the Office of Science and Technology under Dr Lee Dubridge was locked out of the critical stage of preparing the President's budget, the directors' review meeting at which the final trade-offs are made. Carey observed that the OST and OMB are now friends again (if not yet, perhaps, the very good friends they were in his day) and that this year the OST was allowed back into the budget decision process. Nevertheless, relations have not been restored without price. Carey senses a change in the OMB's view of the National Science Foundation—"Instead of the NSF being regarded as a bottomless pit into which one dropped infinite sums of money without expecting even so much as a splash, the NSF today is being led closer towards the marketplace. When I dealt with the NSF there was a revulsion to tainting the NSF with any applied research... the present Administration has come to the decision that research ought to be guided towards pay-offs".

As if to bear out Carey's description of the applied role the Administration sees for science, Dr Edward E. David

devoted the burden of his speech not to the state of science or intricacies of science policy but to saying how sad it will be if the supersonic transport is not built, at least in its experimental version. It is unusual for a President's science adviser to voice views so radically at variance with those of the scientific community on any issue, and to do so with such political partisanship. This perhaps is another part of the price paid for the OMB's friendship. David has once before entered the lists in support of the SST. After the debate on December 3 last year in which the scientific arguments against the aircraft weighed heavily in Senator Proxmire's victory over the Administration, David rounded up a list of no less than 30 scientists, including Dr Edward Teller, who put their name to a statement reproving the Senate for its action. "Our society must not suppress technological advances", David's scientists told the Senate, "but through research, development and experimentation make sure that those advances are obtained without undesired side effects".

David has now returned to the theme of suppression, but in interpreting the Senate's doubts about the desirability of the SST as signs that society has lost the will to experiment, he may have been carried a little beyond his audience's sticking point. The present situation, he said last week in a paper entitled "The Real Crisis", has led to "a loss of confidence by the public in the wisdom and objectivity of technical people, leading in turn to a loss of courage to authorize new experimentation or to undertake new innovative developments".

David went on to describe as the most serious concern for the nation the "increasing alienation of people in our society from rational ways of thought. I have listened to debates on the SST programme at some length", he added in explanation. Mentioning the *in vitro* fertilization experiments of Dr R. G. Edwards and his colleagues, which recently received some publicity before a Congressional committee, David implied that public concern was also impeding or about to impede biological experimentation. "There are many evidences that society does not believe that technology can be controlled in a rational way. Because of that, society is losing its courage to experiment. This trend leads to disaster, for it divorces our decision makers from reality.... Make no mistake, a limitation on experimentation in whatever cause is the beginning of a wider suppression. When we fail to experiment... we bring the best part of American society as we know it today to a halt. Already we see timidity in new undertakings..."

The only serious aspect of this hyperbole is that next time a President's science adviser cries wolf on these grounds, nobody may listen to him. Under questioning, David was unable to cite any specific instance of society's "failure to experiment", other than the issue of the SST. Although adamant that he was pressing for construction only of the two experimental prototypes, David let it be known that a fleet of SSTs would help create the same kind of cultural links with Africa and Australia as conventional air travel has allowed to grow up with western Europe. "If we are going to forge a world-wide community in this sense, we may need to forge a new mode of travel", David explained.

Besides his advocacy of the SST, David had a few words on the unemployment situation—the OST estimates that there are 30–40,000 unemployed scientists and engineers, a rate of about 3 to 4 per cent. The government is tackling the problem by trying to provide placement and retraining services, by cutting back on the rate of increase of the pool of engineers and scientists, and by increasing the rate at which activity is shifted from defence and aerospace to civilian problems.

As for science policy, David said that the policy implications in the President's annual budget document were the product of a more coherent and consistent science policy programme than usually perceived by outside observers. Although more effort toward a science policy for US research and development is needed, David said, the chief problem is "somehow to avoid smothering the substance of science and technology by the inevitable doctrines and dogmas emanating from any science policy".

CONGRESS

Friend for Science?

by our Washington Correspondent

FOLLOWING a reshuffle in the subcommittee chairmanships of the House Appropriations Committee, Congressman Edward P. Boland has become chairman of the subcommittee on Independent Offices and Housing and Urban Development which decides the budgets of NASA and the NSF (subject only to an upper limit placed by the authorization committee). Although a member of the subcommittee for 16 years, Boland is not known to have any particular interest in science; up until now his energies have been concentrated on housing, public works and transportation.

The limit of the personal information Boland enters in the Congressional Directory is that he is the Democratic Representative of the second district of

Massachusetts. The future wielder of almost absolute power over the budgets of NASA and the NSF is the son of an Irish immigrant from Fahan, County Kerry. A graduate of Boston College Law School, Boland was elected to the Massachusetts House in 1935 at the age of 24 and after three terms in the state house was elected Registrar of Deeds in Hampden County. Returning from war service in the Philippines, Boland is said to have refused \$12,000 back pay (Massachusetts voted to award its office holders half their accumulated salaries) on the grounds that public officials were not a privileged class.

A Roman Catholic and unmarried, Boland was elected to the House of Representatives in 1953 and is now serving his tenth term. Although his constituency, including the town of Springfield, is in the west part of Massachusetts and out of the ambit of the east coast academic community, Boland is closely associated with the Kennedys, accompanied President Kennedy on his visit to Ireland, and during his administration had something of the status of an official spokesman in the House. He has voted in the main for traditional liberal measures, opposing the Sentinel anti-ballistic missile programme in 1968 and the proposal last year to expand the Safeguard system (as Sentinel has now become) to five sites. With 17 other Congressmen he sponsored a resolution in 1968, at a time when there were demands for another 200,000 men to be sent to Vietnam, to the effect that US forces there should not be increased without the explicit consent of Congress.

Boland's chief interests have been in the Model Cities programme, a piece of Great Society legislation under which federal support is provided to refurbish the physical and social milieu of selected cities, including Springfield, Massachusetts, as well as in transportation; he has been outspoken on matters of air safety and has served as chair-



Edward P. Boland.

man of the Appropriations sub-committee on transportation, a post he has now yielded. Boland's recorded pronouncements on space or science are few; in a speech made in 1958 in the aftermath of the launching of Sputnik, Boland declared that "in the present race for survival [the US] cannot afford to lose its vast reservoir of brainpower in the fields of engineering, science and the humanities merely because these young Americans cannot be given a college education".

But these are small crumbs of comfort for those seeking a commitment to science, since every Congressman was saying the same thing at the time. The indications from Boland's remarks at the CASW seminar last week are that he is well informed about science policy, favours a strong basic research effort by mission oriented agencies, and that if he has to wield the knife on NASA or NSF budgets will at least do so conscientiously.

NASA

A Leader Found

by our Washington Correspondent
WHEN a Dalai Lama dies the Tibetan priests search high and low for the newborn child whose features proclaim it to be the true successor to the Lama's throne. The Nixon Administration has been combing the land with equal care to find a successor to Dr Thomas O. Paine, who resigned as head of the National Aeronautics and Space Administration on September 15 to return to General Electric. The new head of the agency was announced this week after long delay and much vacillation in high councils as Dr James C. Fletcher, a 51-year-old Mormon who is president of the University of Utah.

The search for a successor seems to have been inhibited by the lack of appeal of presiding over NASA's contracting budget. At least eight people seem to have been considered with varying degrees of seriousness, including George M. Low, the deputy administrator; Howard W. Johnson, president of the Massachusetts Institute of Technology; Colonel Frank Borman; Congressman Richard L. Roudebush of Indiana; Roger Lewis, president of the General Dynamics Corporation; James M. Beggs, Undersecretary of Transportation; Congressman George Bush of Texas; and Frank Jameson, president of Teledyne Ryan Aeronautical.

Within the last few weeks the choice was narrowed down to Beggs, Jameson and Fletcher, and an article in *Business Week* for February 13 stated categorically that the job had gone to Jameson, described as a "real charger" of a man who would turn NASA to solving the national problems of education, ecology and the environment as well as keeping

the flag flying in space. Unfortunately Jameson fell out of favour with the Lama-hunters in the White House before his appointment could be announced officially and after a few weeks of silence it emerged that the chosen one was not Jameson after all, but Fletcher.



Dr James C. Fletcher

Fletcher is a physicist with industrial experience in several aerospace companies. He served with the Aerojet-General Corporation prior to 1960, as president of the Space Electronics Corporation from 1960 to 1962, and as chairman of the Space General Corporation until 1964, when he became president of the University of Utah. Fletcher has also been a consultant to the President's Science Advisory Committee, the Office of the Secretary of Defense and the Arms Control and Disarmament Agency.

Other recent changes in NASA's personnel include the dismissal at a month's notice of Julian Scheer, assistant administrator for public affairs, who has been with NASA since its earliest days. Architect of the extensive coverage whereby NASA's doings were made known to the world, Scheer is said to have fallen foul of certain congressional interests in his methods of presenting NASA.

ARMS RACE

Accuracy of ICBMs

by our Washington Correspondent
AN order of magnitude improvement in missile guidance is within the grasp of present-day technology, according to Dr David Hoag, director of the Apollo Guidance and Navigation Program. Quite feasible advances in missile guidance techniques could reduce the average error with which an ICBM hits its target, now 1,000 metres at most, to about 30 metres, the chief residual

source of inaccuracy being imperfect knowledge of the Earth's gravitational field.

Improvement of missile accuracies to this degree has alarmed many arms control experts because of the possibility it offers of a successful first-strike attack that would destroy the enemy's missiles in their silos. Hoag, however, in a paper delivered to a Pugwash symposium held at Racine, Wisconsin, last June and now published in book form* argues that the development of high accuracy missiles would encourage the exchange of weapon against weapon instead of weapon against city.

Hoag lists seven major sources of inaccuracy in missile guidance and the approximate errors they cause in the range and track of the missile. One class of error resides in the inertial sensing caused by errors in the accelerometers and gyroscopes. Another may be inaccuracy in specifying to the missile's computers initial conditions such as the location and attitude of the missile on its launcher. A third kind of error may arise from the computations used to track and control the missile's flight. By and large, the trajectory calculations can be made as precise as desired by increasing the number of terms in the power series describing the trajectory, which is limited only by the capability of the on-board computer. Hoag believes that the state of technology in compact digital computers should be able to reduce target-miss contributions from this source to negligible proportions.

Thrust termination errors arise if all the components of the missile velocity fail to attain the exact value required at the moment the rocket thrust is cut off. For example, a 0.1 metre/sec difference between actual and predicted values causes a 650-metre miss at the target. This difficulty can be eased by using an additional low-thrust rocket, called a vernier stage, which adds the last small part of the required velocity. In fact, for a MIRV missile the final vernier-like correction can be made as the individual warheads are released from their "bus".

Fifth, there are the gravity anomalies in the Earth's gravitational field which are too slight to affect missiles at full height but may exert a small influence during the initial and terminal phases of flight. A gravity anomaly altering a missile's velocity by 0.02 metre/sec during the first 100 seconds of launch could cause a 50-metre miss at target.

Uncertainties in the geographical coordinates of the target are a sixth source of error, and finally the missile may be deflected by aerodynamic forces as it re-enters the atmosphere.

* *Impact of New Technologies on the Arms Race*. Ed. B. T. Feld and others. MIT Press, 1971; \$2.95.

NEWS AND VIEWS

Polywater Drains Away

It now begins to seem as if the concept of polywater is on its last legs. The article by Barnes, Cherry, Finney and Petersen on page 31 of this issue of *Nature* is one of several recent demonstrations that many of the observations put forward in the past few years as evidence of the existence of an anomalous form of water must be accounted for quite differently and much more trivially. On the face of things, at least, polywater seems to be not a distinct, stable (or metastable) form of water, but water contaminated by various substances among which silicates seem to be conspicuous. One of the most striking and direct demonstrations of this was the article by Bascom, Brooks and Worthington last year (*Nature*, 228, 1290; 1970), which described how electron probe measurements of the residues obtained by condensing polywater had revealed the presence of silicon and sodium atoms. The quantities are indeed sufficiently small to explain why the presence of impurities has been unnoticed for so long. This, however, does not fully account for the way in which experimenters in the late nineteen sixties were apparently happy to record observations of anomalously high viscosity and boiling point without subjecting their samples to the full rigours of modern microanalysis.

The social significance of this phenomenon is important for the conduct of science as a whole. This, after all, is not the first occasion on which groups of people have been led astray in the pursuit of physical phenomena which later turn out to be unreal. At the turn of the century, for example, there was a great fashion for the study of "N-rays" as they were called, supposed accompaniments of some forms of radioactive decay. All kinds of reputa-

tions were then pinned to the characterization of a phenomenon that turned out to be an illusion. On reflexion, of course, it is ironical that soon after the false doctrine of N-rays had flourished and then disappeared, biologists fiercely resisted the suggestion that some forms of chicken sarcoma could be transmitted by the so-called Rous sarcoma virus. It has taken a long time to show that this scepticism was misapplied. The problem of polywater is more subtle, to the extent that the growth of the concept has required not merely the suspension of belief or disbelief but an unwarrantable restraint on the use of observational techniques which are, after all, familiar enough. The failure of several experimenters to pursue with all the vigour at their command the possibility that contamination might account for most of their observations is nothing to be proud of.

So has the rise and fall of polywater been nothing more than a salutary lesson for everybody, theoreticians as well as experimentalists? Luckily, even unhappy tales like this contain the seeds of useful and constructive speculation. Thus there has been great interest in model building with water molecules, and many theoreticians have been led to construct schemes for estimating the severity of three-dimensional complexes of water molecules which are of great interest in themselves. If, as now seems likely, the properties of polywater are to be explained away by the presence of similar atomic networks in which atoms of silicon play a part, it remains something of a surprise that sheets of atoms constructed in this way can serve so dramatically to modify bulk properties such as viscosity and boiling point.

The DNA Replication Mystery

As a group, molecular biologists have never fought shy of publicity and their propagandists have fed the popular image of the juggernaut subject advancing by enormous strides on all fronts. All fronts bar one, which is usually glossed over; for, although anybody reared on the current rash of molecular biology texts might be excused for thinking otherwise, the precise mechanism by which DNA is duplicated is almost as obscure today as ever.

To be sure, in the nineteen sixties Kornberg and his collaborators isolated from *Escherichia coli* an enzyme which can polymerize DNA and, for example, replicate infectious bacteriophage DNA *in vitro*. And until last year this enzyme, DNA polymerase I, was the only known enzyme with this capability. Small wonder therefore that many molecular biologists bent over backwards devising all sorts of ingenious schemes to explain how it might replicate DNA *in vivo*. But they laboured under one great disadvantage and probably in vain, for the properties of polymerase I (it makes DNA very slowly, it can only synthesize DNA in one, the 5' to 3' direction, and it is

an effective exonuclease) are those expected of a DNA repair enzyme rather than a replicase. In other words, polymerase I has all the properties of an enzyme which edits DNA sequences, excising regions of mismatched base pairs and replacing them with the correct sequence, rather than replicates them.

The days of tortuous scheming are, however, over; the pressure is off polymerase I for two related reasons. First, at the end of 1969 De Lucia and Cairns isolated six mutant *E. coli* which lack or contain defective polymerase I but nonetheless survive and replicate their DNA normally. Second, several groups have detected and begun to isolate a second DNA polymerase, polymerase II, in these mutants. With at least two candidates at last in the lists, those molecular biologists concerned with defining the mechanism of DNA duplication are currently far more intent on characterizing DNA polymerase II and searching for further species of DNA polymerase than on striving to accommodate polymerase I in hypothetical schemes of DNA replication. Few of them would now quarrel with

the statement that the chief and probably sole function of polymerase I is DNA repair. If there are any waverers still sitting on the fence they should be finally decided by what Kelley and Whitfield have to say, on page 33 of this issue of *Nature*, about one of the six mutants isolated by De Lucia and Cairns.

De Lucia and Cairns, when they began their mutant hunt, argued that if polymerase I is a repair enzyme it should be possible to select mutant *E. coli* with a defective repair mechanism, because of a defective polymerase I, but still capable of normal DNA replication. They proved their point by isolating six such mutants, the first of which to be characterized proved to be an amber mutant. But although these strains lacked polymerase I activity and were unusually sensitive to ultraviolet light, there was no proof that the mutations were actually in the structural gene which specifies polymerase I. It might, for example, still be argued that the mutations were in a regulatory gene and result in a drastic reduction in the amount of polymerase I made. If that were the case, the handful of perfectly normal polymerase I molecules might all be sequestered immediately for DNA replication, which would proceed normally, leaving none available for DNA repair.

Because of such arguments it became a matter of considerable importance to find proof that any one of the six mutants De Lucia and Cairns isolated (all those mapped so far are in the same gene) contain a lesion in the structural gene directly specifying polymerase I. For, clearly, if the enzyme molecule itself contains a mutation which reduces

its capacity to polymerize DNA, and cells containing such mutant enzyme replicate their DNA but lack a repair mechanism, the straightforward conclusion is that the polymerase I is involved in repair but not replication. Of course, anybody absolutely determined to keep all options open could say that all the mutations by chance occur in that part of the polymerase I molecule, admittedly a large protein comprising about 1,000 amino-acids, which is involved in repair, leaving the part responsible for DNA replication intact. But that is surely less plausible.

Fortunately as Kelley and Whitfield have now convincingly shown, *pol 6*, one of the half dozen mutants, turns out to be a temperature sensitive mutation. They purified polymerase I from this mutant and wild type *E. coli* and compared the properties *in vitro* of the two enzymes. The wild type enzyme has the same activity at 37° C and 52° C and its optimum temperature is 55° C. By contrast the mutant enzyme had less activity at 52° C than 37° C and an optimum temperature of about 45° C. Further, the two enzymes differ in their response to various synthetic DNAs and, although immunologically similar, sensitive complement fixation tests at various temperatures indicate that they differ in precise conformation. In short, the *pol 6* mutation is in the structural gene specifying the enzyme. Polymerase I is therefore certainly involved in DNA repair, and although it is premature to state categorically that that is the enzyme's sole function, the odds are stacked very high against the idea that polymerase I has a role in DNA replication.

What Turns Plants On?

By the late 1950s, it seemed clear that there is a subtle—at that time, many privately thought mythical—pigment system located within the plant which can differentiate between light and dark; a mechanism which can be “switched-on” by daylight and “switched-off” by the approach of nightfall. In 1959, the doubts of even the most sceptical of biologists were stilled when W. L. Butler and his colleagues at the United States Department of Agriculture Research Station, Beltsville, Maryland, used a modified difference meter—a machine derived from the Chance spectrophotometer and designed originally to test for ripeness of fruit—to demonstrate the physical presence of the pigment phytochrome in plant tissue. Since then, phytochrome has been extracted from a great variety of plant material. It has been partially purified, and has been shown to be a blue-green chromoprotein, perhaps rather similar to the algal chromoprotein allophycocyanin.

At present, opinion is sharply divided about the exact mechanism of phytochrome action. On one hand, the Beltsville group have proposed the activation of some cellular metabolite or membrane component. In an article published last year in *Nature*, Harry Smith extended this idea to suggest that phytochrome might be located within certain critical membranes, and that its function might be that of a transpermease; a molecule responsible for ferrying important metabolites across the membrane (*Nature*, 227, 665; 1970). On the other hand, for some years now Hans Mohr and his group at Freiburg have been assembling data to support the idea that phytochrome, when switched on by light, activates specific genes which had been only potentially active while the plant remained in darkness.

The enzymes formed as a consequence of this gene depression are then responsible for initiating the structural changes which mark photomorphogenic development in the light.

There is good evidence to support both of these ideas, and very little to suggest that either hypothesis should be discarded. What Professor Mohr proposes on page 56 of this issue of *Nature* is not a new scheme to explain the mechanism of phytochrome action, but the equally valuable suggestion that there is now enough experimental evidence to indicate to plant physiologists that they must be less rigid in their way of looking at the problem of photomorphogenesis. In particular, he asks, what justification is there for the widespread assumption that the primary reaction of phytochrome—the first reaction in which the phytochrome molecule participates after its activation by light—is the same in all plant species, all cell types and in all parts of the same cell?

The idea that there is but one primary reaction in the phytochrome system must have sprung originally from the vast range of plant photoresponses in which the pigment is implicated. These include time dependent responses—flowering, circadian rhythms, re-setting of the biological clock—and time independent responses—pigment formation, enzyme synthesis, de-etiolation, tropisms. All of these responses involve a large amplification factor, that is, the amount of light energy required to initiate the process is small compared with the energy required for the final display. It seemed likely therefore that phytochrome must act at some very early common point in the metabolic processes causing these phenomena. Professor

Mohr has now re-examined his data for the phytochrome regulated synthesis of anthocyanin and ascorbic acid in the white mustard seedling, using sequential irradiations with red and far-red light. Briefly, red light promotes the formation of the apparently active form of the phytochrome molecule, PFR, and thereby initiates biochemical synthesis. Far-red light, however, abolishes this response by converting PFR back to the inactive form of the molecule PR. In continuous far-red light, however, enough phytochrome is converted to PFR to initiate and maintain the synthesis of anthocyanin and ascorbic acid. This property of the phytochrome molecule makes it possible to perform some pretty tricks with red and far-red light. The way the plant reacts to these colours of light given in different amounts and in different sequences makes it possible to gain some insight into what the phytochrome molecule is doing in the cell.

Mohr and his colleagues show that in continuous far-red light, PFR exists in an, as yet, unexplained "excited" state, which promotes the formation of both anthocyanin and ascorbic acid. With the passing of time, however, it becomes clear that this "excited" state is not the same for the two synthetic systems. After twelve hours of continuous far-red light, red light greatly promotes the synthesis of anthocyanin, but is completely ineffective in promoting further synthesis of ascorbic acid. What Mohr deduces from these results is that the PFR formed under far-red light takes part in different kinds of primary reactions in promoting synthesis of the two compounds. In one case the molecule is still susceptible to further conversion by red light, but in the other the active molecules have become in some way immune to further photoconversion.

Important and unequivocal as this conclusion is, there will inevitably be much head shaking and anguish in the photomorphogenesis camp. The attempt to isolate one primary reaction for phytochrome has proved challenge enough; what hope is there of identifying similar involvements? There is not much hope at present, perhaps, but at least Mohr's conclusions should make it rather simpler to explain those paradoxical experimental results where the behaviour of phytochrome as observed in the difference meter seems to bear no relation at all to the physiological responses of the plant.

EXPLORATION

Sacrifice for Plants

by our Botany Correspondent

WHATEVER may be said about the boredom of botany, its exponents used to be among the most reckless of adventurers, facing disease and danger in the search for new knowledge. Among the most intrepid of plant hunters were Sir Joseph Banks and Daniel Solander who sailed to Australia two hundred years ago with Captain James Cook on his ship *Endeavour*. The plants which they collected on this voyage became the basis of the department of botany of the British Museum (Natural History) where they are still in constant use. Banks's great contribution to botanical knowledge was recalled on February 23 by Dr W. T. Stearn when he gave the second of six weekly lectures about the Natural History Museum, which have been sponsored by the museum and the Victorian Society.

Banks, a rich landowner who had been to Eton, Harrow and Oxford, showed sufficient promise as a botanist to be elected a Fellow of the Royal Society at the age of 23. Deciding that a trip to the South Seas would provide a good opportunity for a grand tour of the world, he persuaded the society to let him go with Cook on his voyage to observe the transit of Venus. Banks also enlisted the Swede Daniel Solander, an extremely erudite pupil of Linnaeus, and Sydney Parkinson, the artist who died during the voyage.

The first important collections of the voyage were made during secret midnight excursions at Rio de Janeiro, where the Portuguese viceroy would not let Cook and his men ashore. In Tahiti, where the welcome was much more friendly, they found a rich vegetation, especially orchids, but Banks was most impressed by the breadfruit plant. From Tahiti, where the transit of Venus was duly observed, the expedition moved south, and, not finding the great southern continent which some people had imagined to be there, pressed on to New Zealand. Here a large collection of specimens was assembled, including the shrubby veronica, *Hebe*. Banks's foresight in taking along a Tahitian priest who could act as interpreter enabled the explorers to make friendly contact with the Maoris, who had attacked their previous European visitor Abel Tasman.

Endeavour then proceeded to Australia, known then as New Holland, the west side having been explored by the Dutch. Cook and his comrades found an inlet on the eastern coast which the captain would have called Sting Ray Bay, but which was so rich in floral delights that it was named Botany Bay. *Acacia* and the shrub which was later to be called *Banksia* were two of the many plants they found there. After leaving Botany Bay, Endeavour struck a coral reef and had to put into a nearby river inlet for repairs.

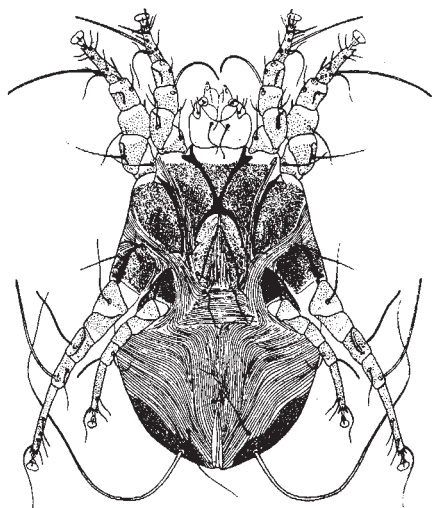
At the end of six weeks when the work was complete, the botanists had become familiar with every aspect of the surrounding flora and fauna. They saw a kangaroo, which was then unknown in Europe, and was to be classified by a German scholar as a new species of mouse, *Mus kangaroo*. Dr Stearn visited this part of Queensland last year, and found its vegetation virtually unchanged from the descriptions of Banks and Solander. He advocates that this region around Cooktown should become a new national park.

After many privations, particularly in disease ridden Java, where Parkinson and other crew members died, the survivors arrived home, and Banks established his home in London as a scientific centre. Sadly, the book he had planned as a record of the voyage, to be illustrated by Parkinson's drawings, was never finished. But his collections passed to his colleague Robert Brown, an army surgeon turned botanist, who added to them during his own travels. Brown, whom Dr Stearn described as the greatest botanist Britain has produced, went on the next British voyage to Australia, which was led by Flinders. He was accompanied by the Viennese artist Ferdinand Bauer, and they brought back a wealth of specimens and drawings. At the same time, Bauer's brother Franz settled at Kew, where a collection was growing up to rival that started by Banks. Brown gave his collections to the British Museum and became the first keeper of the department of botany, from which was to grow the separate Natural History Museum.



Chaitaea tacca (*Tacca leontopetaloides*) collected from Tahiti and drawn by Parkinson in 1769. The drawing is now in the department of botany of the British Museum (Natural History).

ACAROLOGY

Banana Mite

Eight specimens, all female, of an unknown species of mite were collected in 1968 from some bananas in Guatemala by Dr D. Johnson, of the US Department of Agriculture. These mites have since been examined by Dr A. Fain (Institut de Médecine Tropicale, Anvers) and Dr G. W. Wharton (Ohio State University) who conclude that they belong to a new genus and species of the subfamily Dermatophagoidinae (*Bull. Inst. Roy. Sci. Nat. Belg.*, 46, No. 27; 1970). So far only the female of the species is known. This figure shows the ventral view of the new mite, *Guatemalichus bananae*, the body of which measures 450μ by 293μ .

RIBOSOMES

Protein Count

from our Molecular Biology Correspondent

THE perennial question whether all ribosomes in a cell are alike or differ in their protein composition rears its head yet again. Voynow and Kurland (*Biochemistry*, 10, 517; 1971) earn admiration for their perseverance in examining once more the stoichiometry of the 30S *Escherichia coli* ribosome. They have noticeably modified their position as stated in their paper of last year, though the presence of one molecule in each particle of at least a large number of the twenty-one recognized ribosomal protein species remains unchallenged.

Two procedures have been used for estimating the relative concentrations of labelled protein components separated by electrophoresis in polyacrylamide gels. One is direct counting of the sliced gel, and the other, which is advantageous for incompletely resolved components, is an isotope dilution method involving the addition of single pre-purified proteins, in known concentration to the mixture before electrophoresis. The two methods give good enough agreement but the interpretation of the results leaves room for a fairly generous element of personal preference. The

reason is obvious enough; the ribosome will tend to pick up any basic proteins with which it chances to come into contact, for example after lysis of the cells. Such externally adsorbed species can be removed by washing with solutions of moderately high salt concentration, but at some point, or perhaps progressively, ribosomal proteins—those that reside and function in the ribosome *in vivo*—will also be extracted. The treatment that Kurland and his colleagues have used to purify ribosomes tends to the severe, as Voynow and Kurland now implicitly concede. The ambiguity is partly resolved by a comparison of the content of individual proteins of their scrubbed ribosomes, and what they refer to as crude—or, as some other workers would suggest, undamaged—ribosomes.

Referring to washed ribosomes, Voynow and Kurland divide the eighteen proteins that they are able to separate into three categories—unit proteins, which are present to the extent of one molecule per ribosome, fractional proteins, and those on which they are not prepared to commit themselves. There are only six in the first group, but seven in the second. In the unwashed ribosomes at least two of the latter are present in molar proportion. The most satisfactory feature of the results is that the six unequivocal unit proteins are Nomura's core proteins, which are all indispensable for the correct assembly of the 30S ribosome from its constituents. There are no proteins present to the extent of more than one molecule per ribosome.

In favour of the view that certain proteins are indeed present only in some

of the ribosomes is the numerical argument, whereby the sum molecular weight of the twenty-one proteins and the 16S RNA is above the accepted value for the molecular weight of the 30S ribosome—though this again plainly depends on the manner of washing.

Light on the same problem comes from the work of Warner on the kinetics of assembly *in vitro* of the 80S ribosomes of yeast. He finds (*J. Biol. Chem.*, 246, 447; 1971) that all the proteins in the 40S subunit are between 10,000 and 30,000 molecular weight, whereas in the 60S subunits they range up to about 46,000 (as estimated by detergent-acrylamide gel electrophoresis). With a pulse label, after a two-minute interval, the electrophoretic pattern of radioactivity shows some quite gross differences from that after 40 minutes, indicating either that the pool sizes of different ribosomal proteins differ markedly, or that there is a transient difference in the distribution of proteins in the cytoplasmic ribosomes. Evidence in favour of the second explanation comes from experiments with temperature-sensitive mutants grown at restrictive temperatures, which still show the emergence of the transient rapidly labelled components. As in other eukaryotic systems the newly synthesized ribosomes appear in the cytoplasm as subunits, and are only after a long interval, associated perhaps with uptake or loss of proteins, incorporated into polysomes. Warner suggests that the changes with time in the distribution of protein components of the ribosome may well reflect functional entities such as protein synthesis "factors", or for that matter fractional proteins such as those of *E. coli*.

Heterogeneity of Normal Fibrinogen

THE recognition of the existence of genetic variants of clotting factors VIII, IX, X and fibrinogen is of profound importance in the study of blood coagulation. Most clotting factors, with the exception of fibrinogen, are present in only trace amounts in plasma and the finger printing of fibrinogen molecules is a necessary prerequisite to the study of other clotting factors, at the molecular level.

The article by Gaffney in next Wednesday's *Nature New Biology* provides additional evidence for the heterogeneity of normal fibrinogen. Using acrylamide gel electrophoresis of the unmodified fibrinogen they obtained only a single band but, after sulphite cleavage of the fibrinogen, separation with the α (A), β (B) and γ chains was effected. The three chains of sulphitolyzed single donor and pooled fibrinogens were subdivided further into discrete bands with the new technique of isoelectric focusing (IEF) acrylamide. The sulphitolyzed fibrinogen from a single

donor gave fewer bands of differing intensity and location.

This is not the first piece of work which suggests the existence of different normal fibrinogen molecules, and the author's previous work on prolonged plasmin digestion of single donor fibrinogen provides supporting evidence (Jamieson and Gaffney, *Biochim. Biophys. Acta*, 154, 96; 1968). These techniques are less definitive than the finger printing techniques of Blomback *et al.*, who demonstrated an amino-acid substitution and deletion in the α chains of normal fibrinogen (*Nature*, 218, 130; 1968). Blomback *et al.* also showed that the N terminal part of the α (A) chain of the abnormal "fibrinogen Detroit" had arginine residue 19 of a normal α (A) chain replaced by a neutral amino-acid, probably serine (*Nature*, 218, 134; 1968). It will be of interest to study the IEF acrylamide pattern of several single donor normal fibrinogens after sulphite cleavage as an aid to polymorphism studies.

CHEMICAL CARCINOGENS

Smog Transformations

from our Cell Biology Correspondent

CONSERVATIONISTS, especially those whose chief concern is urban air pollution, should read the latest issue of the *Proceedings of the US National Academy of Sciences* (68, 445; 1971). Huebner's group report that extracts of city smog induce the transformation of rat cells infected, but not transformed by mouse leukaemia virus, hamster cells carrying the so-called hamster leukaemia virus and hamster cells apparently free of any tumour virus. Their concluding remark, "a relatively small volume of smoggy urban air may contain a tissue culture transforming dose of smog residue", is of the sort which will be widely aired no doubt in the forthcoming battles over cleaner automobile engines.

It is well known that urban air contains carcinogenic materials but, in the past, assays for these substances have been tediously slow because they have involved painting smog extracts on mice or other suitable animals and waiting for a tumour to develop. The advantages of the *in vitro* assay Huebner and his colleagues have devised are that it is quick, comparatively inexpensive and sensitive. In essence they have exploited their earlier finding that certain chemical carcinogens act synergistically with tumour viruses. A cell exposed to both virus and carcinogen is transformed whereas exposure to either virus or carcinogen alone does not result in transformation. Having filtered some 1.1×10^7 m³ of air from what they discreetly describe as "a central location in one of the major metropolitan areas of the United States", Huebner and his colleagues extracted with benzene from the filters a residue, the concentration of which in air is about 16.2 µg/m³. Each gram of this residue contained the equivalent of about 23 µg of benzopyrene (BZP) and the doses used in the experiments were therefore measured in unit equivalents of pure benzopyrene.

Cultures of rat embryo cells free of leukaemia virus were not transformed by doses of smog residue up to 0.007 µg BZP units/ml., neither were they transformed by doses of up to 1.0 µg/ml. of pure benzopyrene. Cultures of similar cells infected but not transformed by Rauscher murine leukaemia virus were, however, transformed after exposure to doses of residue as low as 0.0007 µg BZP units/ml. Essentially similar results were obtained in similar experiments with hamster cells carrying hamster leukaemia virus. Furthermore, cultures of hamster cells free of any RNA tumour virus were also transformed by doses of 0.007 µg BZP units/ml. but not by doses ten-fold less concentrated.

As Huebner and his colleagues note, the smog residue contains some transforming agent about six hundred times more efficient than pure benzopyrene.

They also point out that the transformed cells produced by exposure to smog residue have not so far induced tumours when transplanted into suitable recipient animals and the results of their *in vitro* tests have yet to be correlated with a demonstration that the residue is carcinogenic for intact animals. Assuming, however, that that will be done, their procedure promises to be a useful method for assaying the carcinogenic potential of chemicals from a variety of sources as well as smog.

NUTRITION

Asian Diet

from a Correspondent

SINCE the first international congress of nutrition was held twenty-two years ago, the successive triennial meetings have become larger and broader in scope.

Smaller meetings, dealing with the more specialized problems of a geographic region or of a particular aspect of nutrition, have therefore become increasingly popular. The most recent of these, the First Asian Congress of Nutrition, held in Hyderabad from January 27 to February 3, was also the first at which the related themes of nutrition and of national development were discussed against the background of high population density and almost universal dependence on cereals as sources of both protein and calories, which typify this portion of the developing world.

In order to formulate a nutrition policy, a correct diagnosis of the nature of the nutritional problems of a country is obviously essential, and much lively discussion at the meetings centred around the implications of the most recent estimates of the requirements for protein of the vulnerable groups of the population,

Ribosomes of Mouse—Hamster Hybrids

HYBRID cells, obtained by inducing with inactivated Sendai virus the fusion of two or more different cells, are proving to be extremely useful material for investigating the control of gene expression in animal cells. The latest example of their exploitation to this end appears in next Wednesday's *Nature New Biology* in which Stanners, Eliceiri and Green describe the analysis of the ribosomes in mouse-hamster hybrids. In extracts of the cytoplasm of these hybrids they have found both hamster and mouse ribosomes. This situation is in marked contrast to their previous findings that mouse-human hybrids contain only murine ribosomal RNA even though the particular hybrids they studied retained as many as thirty-five human chromosomes including those believed to carry the genes which specify human ribosomal RNA. Taken together, the new and old data indicate that it is unwise to generalize about the pattern of expression and repression of the mixed genomes in hybrids.

Human and mouse ribosomal RNAs can be distinguished by their different electrophoretic mobilities. Hamster and mouse ribosomal RNAs, by contrast, have more or less identical electrophoretic mobilities, and therefore Stanners and his colleagues had to search for some other criterion to distinguish mouse and hamster ribosomes. They have exploited Reader and Stanners's observation that hamster ribosomes, not actively translating a messenger RNA, tend to form dimers whereas free mouse ribosomes remain monomers. The question of whether or not hamster ribosomes are made in mouse-hamster hybrids boils down simply to an analysis of the occurrence of monomeric and dimeric free ribosomes in the hybrid cells.

It turned out that the hybrid cells

yielded both monomeric and dimeric ribosomes. Further, the dimers could be disaggregated by heating to 37° C, but when cooled to 0° C they reassociated to yield dimers but never any larger aggregates. Working on the assumption that the dimers are indeed hamster ribosomes, Stanners and his colleagues tried to establish whether dimerization occurred through the ribosomal RNA rather than the ribosomal protein. If the former is the case all the RNA in the dimers should be hamster specific and by labelling parallel cultures of the hybrids with ¹⁴C or ³H uridine Stanners *et al.* have been able to confirm that, allowing for cross contamination, all the 28S RNA in the dimers is indeed hamster specific. They have also shown that a 40S RNA can be extracted by appropriate techniques from the dimers. This RNA is, they believe, a complex of two hamster 28S RNAs, into which it can be dissociated.

In short it seems that free hamster ribosomes are able to form dimers because of an affinity between the 28S hamster ribosomal RNAs; furthermore, the observation that aggregates of more than two hamster ribosomes are never found suggests that each 28S RNA contains only one dimerization site. In mouse-hamster hybrids, transcription of the ribosomal RNA cistrons is not restricted to those of only one parent. It would be interesting to know whether there is any preferential transcription of ribosomal RNA cistrons in hamster-human hybrids and in other hybrids obtained by fusing hamster cells with cells of other species. The curious phenomenon of dimerization of hamster ribosomes is clearly going to provide a simple marker for studying the synthesis of species-specific ribosomal RNAs in many different types of cellular environment.

and of the metabolic interrelationships between protein and energy utilization in the body. Thus, Dr P. V. Sukhatme (Food and Agriculture Organization, Rome), in reviewing estimates of food consumption in relation to income, drew the conclusions that, first, malnutrition is chiefly associated with poverty, that is to say with an uneven distribution of food resources within communities rather than an overall deficit of production at the national level; second, that the major deficiency in India and most other Asian countries is that of calories rather than of protein; and third, that food and nutrition planning should aim at eliminating the deficit by improving both the supply and distribution of foods of the same kind as are currently being eaten. The cereal-pulse based diets of the region are sufficiently well balanced in respect of their protein and amino-acid contents to meet the requirements of infants during the transition from breast feeding to a solid diet. This diagnosis, based on the intakes of food by households, was supported by a review by Dr Ambalini Bailur (National Institute of Nutrition, Hyderabad) of individual intakes of more than 30,000 pre-school children in six regions of India. In all cases, the diets eaten contained an adequate proportion of protein making due allowance for its quality, but the amounts eaten were inadequate to meet energy needs in 92 per cent of the children.

The prospects for improving the nutritive value of food grains were discussed by Dr M. S. Swaminathan (Indian Agricultural Research Institute, New Delhi). Level of yield, of protein and of lysine content can all be influenced by genetic selection. Thus the scope of the "green revolution" can be extended both by these techniques as well as by the adoption of varieties that lend themselves to multiple cropping.

The complexity of the interrelationships between nutrition and infection was reviewed by Dr N. S. Scrimshaw (MIT, Cambridge, Massachusetts). Experience in Guatemala has emphasized the need for an integrated approach to nutrition and public health programmes.

In developing countries, where resources are limited, a clear assessment of priorities is an essential prerequisite for the formulation of nutrition policies. The problems involved in this were discussed by Dr J. L. Joy (University of Sussex), who stressed the need for a re-examination of the nutrition problems of developing countries, which should be seen as caused not so much by deficits in the supply of food either in respect of quantity or quality, but rather as the result of inequalities in the distribution of demand (purchasing power) or of resources (land, water and the like) among the population. An increase in total food production may be necessary for improvement of nutrition, but by itself will never be sufficient.

POLLUTION

Problems with SO₂

from our Geomagnetism Correspondent

THE obvious way to reduce atmospheric pollution is to cut the pollutants off at source—cut down the pollutants by half, perhaps, and the pollution will be halved likewise. But if a recent study by Golden and Mongan (*Science*, 171, 381; 1971) is anything to go by, this apparent truism turns out to be a gross oversimplification. A study of atmospheric diffusion over Cook County, Illinois, has shown that although electric power plants burning fossil fuels contribute about 78 per cent of the sulphur dioxide emission into the atmosphere, they account for only 14 per cent of the sulphur dioxide at ground level. It follows that the complete elimination of power plants, or at least the complete elimination of their SO₂ emissions, would make little difference to the exposure of the population to this corroding chemical.

The basis of Golden and Mongan's analysis is a model—the Air Quality Display Model (AQDM) developed by the National Air Pollution Control Administration—and to this extent it rests on assumptions which are not easy to verify. On the other hand, it is a tried and tested model. The AQDM permits the calculation of annual average concentrations of contaminant at specific points at ground level as well as the percentage of the concentration contributed by each point or area source.

When these calculations were made for data from Chicago, for example, the results were reasonably consistent with observed concentrations of SO₂ during 1968. Golden and Mongan are thus convinced that the model is a good first approximation in areas with level topography and so should give sensible results for Cook County.

Why then do power plants contribute so little of the SO₂ concentration at ground level? Ironically, it all seems to be the result of a primitive pollution control measure adopted many years ago before pollution became the matter of great public concern it is today—the tall chimney stack. The whole object of the tall stack is to avoid high concentrations of SO₂ and other pollutants in the vicinity of the power plant by diffusing them over a wide area and by ensuring that they are highly diluted before reaching ground level. Of course, there is always the possibility that unfavourable meteorological conditions will occasionally defeat the object of this exercise. For this reason the power plant is likely to remain an important target for control. Unusual conditions apart, however, it should be possible to site power plants within a given region in such a way that the yearly average concentration of SO₂ at any given point is lower than the maximum considered desirable by the community in question. Where human health is concerned, Lave and Seskin (*Science*, 169, 173; 1970) have concluded that "it is the minimum level of air pollution that is important, not the occasional peaks". The power plant

Magnetic Fields in the Crab Nebula

THE nature of the magnetic fields revealed by the structure of the Crab Nebula is of great interest, because of the link between these fields and the underlying pulsar. There is no other known system which contains a pulsar young enough for the gases left by the supernova in which it was created to be still visible, so the Crab offers a unique testing ground for pulsar models. Last January, Forman and Visvanathan offered an interpretation of the way in which light coming from this nebula is polarized. They found a smooth variation in this polarization (*Nature*, 229, 39; 1971), but in next Monday's *Nature Physical Science* J. D. Scargle, who has been associated with observational measurements of polarization in the Crab, points out that interpretation of these data must be approached with great care.

The chief difficulty affecting the observations is the different thickness of nebula in the line of sight at different distances from the centre—assuming a spherical distribution, light is seen from a much greater quantity of gas when the centre of

the gas is looked at closer than the edge.

Scargle suggests, however, that the observations show a roughly elliptical cavity immediately around the pulsar, just as the sweeping action of the rotating strong magnetic field linked to the pulsar might suggest. This means that radiation seen to be coming from regions in the line of sight close to the pulsar originates only in the part of the surrounding shell outside this cavity, and it provides no direct information about the field close to the pulsar.

It is possible that the observations are consistent with the radiation of particles in the magnetic field produced by a toroidal current loop viewed nearly edge on. But it remains to be seen whether this model will survive more detailed observations. It is difficult to construct theories of the nature of the wisps of matter in the Crab because they provide such a radical departure from the average conditions around them, but their link with the glitches (sudden jumps in period) of the pulsar should help to provide a complete model eventually.

thus emerges as not quite the boggy it is often assumed to be, but a boggy nonetheless.

The really important source of SO₂ is domestic and industrial heating—that is, small localized sources related to the distribution of population and industry. Evidence for this comes from the variation of SO₂ concentration throughout the year. In cities concentrations are usually higher in winter than in summer in spite of the fact that emission from power plants is the same throughout the year or even higher in summer. Golden and Mongan thus suggest that as a matter of policy, where low sulphur-bearing fuels are available they should be directed to small emission sources in highly polluted areas rather than to the bigger power plants. In many areas of Britain, however, the situation may be a little different. In regions where the Clean Air Act has been applied the proportion of SO₂ at ground level attributable to small localized sources is probably smaller than in Illinois.

RADIO GALAXIES

Dichotomy Confirmed

by our Cosmology Correspondent

OPTICAL spectra of nearby giant E and SO galaxies have been obtained by M. J. Disney and R. H. Cromwell at the Steward Observatory in Arizona. The spectrograms show a definite correlation between radio and optical activity in the galaxies (*Astrophys. J. Lett.*, **164**, L35; 1971). Earlier surveys of nearby galaxies have revealed that twenty-four of our near neighbours are radio sources. Most of these are fairly weak, but some are sufficiently strong that it has been suggested that they have quasars at their nuclei (Heeschen, *Astrophys. J. Lett.*, **6**, 49; 1970). These are known as active sources; the quieter, non-quasar-like radio galaxies are referred to as passive sources. This classification is far from being an arbitrary rule of thumb, because the active sources are all unresolved (radio diameters less than 1.5 arc sec) and the passive sources are extended over more than 1 arc minute, and show the "normal" inverse power law spectrum, indicating a thermal origin for the radiation.

The apparent similarity at radio wavelengths between some nearby galaxies and the general run of nearby quasars has been an attractive feature of these radio observations. However tenuous the link might be, many astronomers would be relieved to find a way to tie quasars into the normal evolutionary pattern of galaxies. Since Heeschen's work, these twenty-four nearby galaxies have been obvious candidates for optical study.

The latest survey, carried out on twelve of these radio galaxies, confirms this dichotomy. Disney and Cromwell found that the spectral properties of these

galaxies are related to their radio properties, but not in the way that might be expected on a naive quasar nucleus assumption.

Three of the galaxies studied by Disney and Cromwell have normal radio spectra, eight have the radio characteristics defined by Heeschen as active, and one seems to be an in-between case. All of the normal radio galaxies also show normal optical spectra. All but one of the active radio galaxies show emission lines of the type characteristic of planetary nebulae, and some of these lines are very strong. Although the sample of galaxies is rather small, the significance of this correlation is clearly unquestionable. Interestingly, the one active galaxy which does not show emission lines (NGC 4552) has already been shown to have an abnormally highly reddened centre, so that any emission lines originating in the nucleus might be obscured by matter

around the centre of the galaxy (see, for example, Tifft, *Astr. J.*, **66**, 390; 1969).

In one of the spectrograms studied, that of the galaxy NGC 3998, the hydrogen alpha line is seen clearly, and has broad extended wings of the kind seen in the emission from quasars at this wavelength.

But unfortunately for the quasar nucleus model, the activity revealed by these spectrograms is not the kind normally associated with quasars. In general, quasars have strong, broad hydrogen lines, which are not seen in any of these nearby galaxies, except NGC 3998, and the excitation (measured by the relative intensity of 5007 [0111] to 3727 [011]) is much lower in these galaxies than in quasar or Seyfert galaxies and is more typical of planetary nebulae. Although it is now clear that Heeschen's classification really does define two separate families of galaxies, the processes differentiating between them remain obscure.

Explaining Away the Anomalous Background

GOING by measurements at microwave wavelengths, the temperature of the universe is fairly certainly 2.7 K. This has been the consistent result since Penzias and Wilson in 1965 reported the detection of an isotropic background radiation at microwave wavelengths, apparently pervading the universe, which most likely is the fading glimmer of the flash that accompanied the start of the big-bang universe. But at wavelengths shorter than a few millimetres it is impossible to observe this radiation from the ground—the atmosphere is in the way. So do the surprisingly high fluxes that have recently been measured in the far infrared during rocket flights mean that something is wrong with the rocket experiments, or that the measurements at longer wavelengths cannot be extrapolated to the far infrared along the 2.7 K blackbody curve, or what?

The problem is important because the existence of the background radiation is the strongest card held by the proponents of the big-bang cosmology. It is extremely hard, for example, to account for the radiation as a result of the superposition of radiation from distant galaxies—it has to be that astronomers are recording something *primaeval*. But it would throw a spanner in the works if the radiation turned out not to follow the blackbody spectrum. Until there is an explanation of the anomalously high fluxes corresponding to 8 K which have been recorded in the far infrared on two occasions now, the big-bang cosmologists cannot be happy with their trump card.

The possible loophole is that the two rocket measurements covered a range of wavelengths, from 0.4 mm to 1.3 mm. Could it be that the radiation, in fact, follows the 2.7 K blackbody curve, but that superposed on the curve are spectral

lines which lead to an average temperature of 8 K? There have been several hints that this could be so. There has been observational work which places upper limits on the temperature at a number of specific wavelengths in the 0.4–1.3 mm interval which are less than 8 K. One possibility that would fit is that there is a line between 0.8 and 1 mm. A report in next Monday's *Nature Physical Science* describes the results of such a search for lines in this interval and on either side.

Beery and his colleagues have been able to carry out their measurements from the ground by taking advantage of the low atmospheric noise at the Mauna Kea observatory, Hawaii. What they have done is to look for lines using the 24-inch telescope, and attempt to ascertain whether or not the lines are atmospheric in origin (by seeing whether the line strengths depend on the air mass observed by the telescope). They particularly draw attention to a line on the long wavelength side of 0.8 mm which conceivably has a bearing on the far infrared measurements. Most likely the line is attributable to N₂O, however. It seems to be atmospheric therefore, making it an awkward explanation of the rocket measurements, although it might account for recent balloon observations by Muehlner and Weiss (MIT) of an anomalously large far infrared flux.

It is probable then that the team working at Mauna Kea have drawn a blank. All the same theirs is a useful negative result—it is the first high resolution search from the ground for isotropic emission features in this spectral region, and it fixes valuable limits for explanations of the anomalous flux. But it looks as if the astrophysicists will have to look elsewhere for a solution.

NUCLEATION

That Factor 10^{17}

from our Molecular Physics Correspondent

THE strange controversy over the existence or non-existence of a factor 10^{17} in homogeneous nucleation rates seems to be dying down, leaving everybody not a little the wiser and more respectful of the subtleties of surface thermodynamics and the interplay of energy and entropy in very small systems. It is now nearly ten years since Lothe and Pound (*J. Chem. Phys.*, **36**, 2080; 1962) threw their carefully poised spanner into the works, arguing that the classical Becker-Döring-Volmer-Zeldovitch treatment of the dynamics of droplet formation contained a false prescription for the handling of translational and rotational degrees of freedom when notionally dispersing a bulk liquid into nuclei of almost atomic dimensions. The sheer size of the discrepancy introduced—about 10^{17} in steady-state concentrations, but logarithmically less in terms of critical supersaturation—indicated that something more than a poor approximation was to blame, though the issue was confused by other dubious factors such as the use of a “surface tension” to write the free energy of even the smallest nuclei. Lothe and Pound, with somewhat more courage of conviction than modesty, indicted the classical theory on the particular count that it failed to represent the considerable entropy due to the Brownian motion of clusters, both translational and rotational. They then applied what seemed to be the obvious correction for this, obtained the factor 10^{17} for typical cases and accepted quite cheerfully that it destroyed agreement with the, admittedly not impressive, experimental data on critical supersaturations available at the time.

Since then, argument has ranged back and forth in the pages of the *Journal of Chemical Physics* and elsewhere, at times seeming only to deepen the confusion, more recently shifting, it seems, decisively, in favour of the chief antagonists of the Lothe-Pound position, H. Reiss and J. L. Katz of UCLA. Now two important constructive reviews of the subject have appeared (H. Reiss, *J. Stat. Phys.*, **2**, 83; 1970, and P. P. Wegener and J.-Y. Parlange, *Naturwissenschaften*, **57**, 525; 1970); the first makes a convincing attempt to say the last word on the translation-rotation paradox, the second reviews this in relation to more recent experimental results. The Wegener-Parlange article, incidentally, contains one of the best bibliographies in existence for this tortuous subject.

In this most recent critique Reiss meticulously reconstructs the basic equation for equilibrium concentrations of N-atom clusters in a supersaturated gas using a very general formulation in terms of the classical configuration integral and

postponing all details of the geometry and internal mechanics of the clusters for as long as possible. The only assumption at this stage, apart from classical mechanics and gross spatial homogeneity, is the existence of geometrically distinct clusters in the condensing gas, corresponding to certain factorizations of the total configuration integral. Reiss then defines the physical clusters by a new collective property, introducing the idea of a minimum sphere enclosing all atoms present to replace the centre of mass used as the locating property in his previous treatments. This seems to be the crucial step in solving the problem, because use of the minimal sphere both allows the centre of mass of the cluster to fluctuate realistically and at the same time leads to a definition of clusters in a form as close as possible to the nature of an actual physical drop. When these assumptions are carried through—a process considerably more intricate than can be rendered here—the result is no longer a factor of 10^{17} but a relatively harmless coefficient or order unity which is later shown to arise quite naturally as a correction for the fact that real clusters are not isolated in their internal motions but exist subject to their own vapour pressure.

Although there are still passages in Reiss's argument where sleight of hand could cover a fallacy, the final result is convincing even without the accumulating weight of experimental evidence with which it now brings the whole theory altogether more into line. This evidence,

much of it arising from supersonic nozzle experiments, is also reviewed by Wegener and Parlange (*loc. cit.*) and is added to in the form of critical supersaturation calculations in a recent article by J. L. Katz (*J. Stat. Phys.*, **2**, 137; 1970) using the Reiss approach. Readers of these columns will also have noted recent activity in the cause of molecular beam cluster studies which seem likely to yield much more direct information about the properties of small growth nuclei in the near future (see *Nature Physical Science*, **229**, 99; 1971).

Although the resolution of the “translation-rotation” paradox now seems to have gone against the original instigators, none of their critics has suggested that the flaw in Lothe and Pound's original argument was in any way superficial; on the contrary it is now generally recognized to have been embedded close to the heart of the central mystery—of how correctly to characterize a surface in micro-statistical terms while making a consistent guess (for it can still hardly be more) at the partition functions for the internal motions of small droplets. The considerable credit due to Lothe and Pound for throwing open the whole question is all the greater because, however dramatic the numerical factor involved, it was logical rather than numerical consistency which was at stake. At the same time Reiss and his collaborators can be well satisfied at having, so to speak, cleared the decks of a nasty theoretical problem in time for the next round of experiments.

Anomalous Water and Silicic Acid

ON page 31 of this issue of *Nature*, Barnes *et al.* clearly state their belief that the properties of anomalous water are no more than the sum of the properties of the impurities present and that anomalous water as such probably does not exist. Some support is given to this contention by the work of Adams *et al.*, an account of which appears in next Monday's *Nature Physical Science*; they claim that the “anomalous water” produced in their experiment is chiefly a silicic acid solution.

Adams and his colleagues have made use of the high surface to volume ratio of porous glass to create the same environment for the production of anomalous water as other workers who have used Pyrex capillaries, for example. They took a number of precautions to reduce contamination which included burning off organic impurities in the porous glass at 550° C in a stream of oxygen and the use of greaseless Teflon valves throughout the apparatus. They also used a quartz iodide lamp to irradiate the porous glass cell on the grounds that strong irradiation would enhance the surface reactions which have been generally thought necessary for the successful production of anomalous

water. Adams *et al.* found that the liquid which condensed after passing through the walls of the porous glass container showed many of the properties of the anomalous water reported last year by Deryagin (*Sci. Amer.*, **223**, 52; 1970).

These properties are not spelt out by Adams *et al.*, but presumably they include high values for molecular weight and boiling point, for example. Desiccation of the liquid samples revealed solid bodies which contained between 10 and 30 per cent of silicon and about 5 per cent sodium by weight according to electron microprobe analysis. They point out that this is consistent with the work of Morariu *et al.* (*Nature*, **227**, 373; 1970), who found similar bodies in samples of anomalous water and conjectured that these solids had condensed from silicic acid solution; unfortunately, they performed no chemical analyses to support their hypothesis. Adams *et al.* suggest that the anomalous water which they produced is almost certainly a silicic acid solution and this goes a long way towards establishing experimentally that the chief characteristic of anomalous water may be that it does not exist.

Psychobiological Studies of Aggressive Behaviour

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Field studies of aggression in primates are only just beginning. Their fruits, however, could have considerable relevance to the human situation*.

ALTHOUGH there are obvious reasons for scientific interest in the evolutionary and developmental origins of human aggressive tendencies¹⁻⁴, there has been little work on this subject. For example, in the upsurge of primate field studies during the past decade, very few studies have paid primary attention to aggressive behaviour⁵⁻⁹. For this reason, Eric Hamburg and I have recently conducted an investigation of aggressive behaviour in chimpanzees and baboons in East Africa. For more than 200 hours we observed animals within ten yards of us, and with help from Jane van Lawick-Goodall in Tanzania and Phyllis Jay in Kenya, we collected considerable data on aggression in these two species.

By "aggressive behaviour" I mean actions in which an animal inflicts, or tries or threatens to inflict, damage on another animal—in other words, threat and attack patterns. I shall try to describe such patterns, to determine the conditions in which they occur, and the circumstances in which they are likely to be diminished or terminated, particularly by means of inter-animal communication.

We chose chimpanzees for our study because they are probably man's closest living relative. There are many similarities between man and chimpanzee in the number and form of chromosomes, in blood proteins, in immune responses, in DNA, in brain structure and behaviour¹³. The more we see of their behaviour, the more we are impressed with the resemblance between some of their communicative repertoire and that of the human species.

Goodall's research at the Gombe Stream Reserve in Tanzania¹⁰⁻¹² has the special assets of detailed, systematic, close range, longitudinal observation with full recognition of individual animals and their habituation to human observers. And her husband, Hugo van Lawick, has made an extensive film record of threat and attack patterns in chimpanzees, and the various contexts which elicit such patterns. He has filmed several types of threat behaviour, ranging from the low intensive threat (a slight tipping back of the head accompanied by a soft coughing sound) to high intensity threat (such as swaying back and forth in an upright position, or charging towards the animal which has elicited the aggressive behaviour). His films also show various attack patterns, ranging from a slap of the extended hand to a vigorous stamping on the back of the victim. Stones and sticks are used as simple but effective weapons¹⁴. Attacks are usually of short duration—on a time scale of seconds or minutes—and, immediately afterwards, the victim often approaches the aggressor with submissive gestures and is then apparently reassured with gestures such as an embrace or pat on the back. Indeed, Goodall has described a remarkable repertoire of submissive and reassurance behaviour patterns.

*This article is based on the first Sir Geoffrey Vickers lecture, given under the auspices of the Mental Health Research Fund in London in February 1970.

Goodall's observations and our own suggest that aggressive interactions are likely in connexion with (1) competition over food, especially if highly desirable foods are spatially concentrated or in short supply; (2) defence of an infant by its mother; (3) a contest over the dominance prerogatives of two individuals of similar social rank; (4) redirection of aggression (such as when a low ranking male has been attacked by a high ranking male and immediately turns to attack an individual subordinate to him); (5) a failure of one animal to comply with a signal given by the aggressor (for example, when one chimpanzee does not respond to another's invitation to groom); (6) strange appearance of another chimpanzee (such as a chimpanzee whose lower extremities became paralysed by poliomyelitis); (7) changes in dominance status, especially among males; (8) the formation of consort pairs at the peak of oestrus (this is quite a recent discovery among wild chimpanzees and requires further study).

Longitudinal study is essential to clarify the nature and extent of aggressive behaviour in the context of changes in dominance relations. These relations may be stable for long periods, but when they change, the level of violence is likely to increase. For example, van Lawick filmed a transition period in which a male first acquired a top ranking status. He was very aggressive, constantly threatening and often attacking his subordinates, but as months passed and his position apparently became more secure, he became increasingly tolerant of other chimps. During the several ensuing years, he has been the highest ranking chimp in this community, and is accorded much deference by the others.

Early Aggression

Goodall has also reported the development of aggressive patterns during the early years of life. For example, a ten month old male infant was filmed making typical threatening gestures, in a context similar to those of adult threat. These early aggressive patterns are much more characteristic of males than of females. Kinship is also important in the development of such behaviour; for example, a juvenile may threaten or attack chimpanzees older than itself if its mother is near and if her rank is higher than that of the victim. Adolescent males are often aggressive towards females when no higher ranking males are in the group, but apparently restrain such behaviour toward females when dominant males are present. As adolescent males mature, they tend to threaten the lowest ranking mature males and so gain admittance to the hierarchy of adult males. In general, adolescence is a turbulent, aggressive period among these chimpanzees.

Why study baboons? There are several reasons: (1) the adaptive success of the baboon-macaque group, having spread widely through Asia and Africa in various habitats; (2) the baboons are the largest monkeys; and (3) the relatively large extent of their ground-living capability, in a type of habitat (savannah) that was probably crucial in the emergence of early man.

Based chiefly on the extensive field work of Hall, Devore, Washburn, Altmann, Rowell, and Ransom, plus our own observations of baboons in two contrasting habitats, it is pos-

sible to summarize briefly the conditions in which baboon threat and attack patterns are likely to occur¹⁵⁻²¹. The species under consideration here is the common olive baboon of East Africa.

In certain circumstances the probability of overt threat behaviour and of fighting is higher than in the other contexts of baboon life. These include protection of the troop by adult males against predators such as lions and cheetahs; resolution of disputes within the troop by adult males; formation and maintenance of consort pairs at the peak of oestrus; attainment of preferred sleeping sites in the trees, particularly in the presence of predators; acquisition of premium foods such as figs, nuts, and bananas, especially when spatially concentrated rather than widely distributed (Figs. 1 and 2); dominance interactions, especially in the presence of premium foods, scarcity of sleeping sites, and females in full oestrus; exploration of strange or manifestly dangerous areas, a function largely of adult males; and contact between different troops, especially if such contact is infrequent.

Time for Aggression

Baboons, like chimpanzees, spend much of the day in peaceful activities, and there is abundant evidence of affectional systems, similar to those analysed by Harlow in rhesus macaque²². And so what circumstances precipitate aggressive behaviour? In trying to answer this question I shall deliberately discuss aggressive behaviour within the species and against other species.

The circumstances which I have mentioned as conducive to aggression in chimpanzees and baboons reveal that much of the behaviour occurs in two general categories: in defence and in obtaining access to valued resources. Within these general categories, various animals, objects or activities may be involved.

My list of situations eliciting threat-attack patterns in chimpanzees and baboons does not include the defence of a fixed territory; our observations are consistent with those of Crook²³: "Comparative studies of forest fringe, and savannah *Cercopithecus* and *Macaca* monkeys, the baboons of the genera *Papio* and *Theropithecus*, the chimpanzee and gorilla do not provide evidence of rigorously defended territories".

Although most higher primates do not live in permanently fixed territories which they defend rigorously, there is a behavioural distinction between currently familiar and relatively unfamiliar territory. A savannah baboon troop may live in an area of 10-15 square miles for some weeks or months before moving to another nearby area. While it is living in a given area, the behaviour of its members tends to take on distinctive features at the fringes of this currently familiar space: they are highly vigilant, threaten readily, and some males move out a considerable distance beyond females and young in exploring the relatively unfamiliar area. An important topic for future research is the contact between groups in such circumstances. The limited data available suggest that there is considerable caution, vigilance, and agonistic behaviour when groups meet, although usually they seem to avoid serious fighting. In some conditions serious inter-group fighting does occur, for example, among rhesus macaque groups in cities in conditions of crowding, food shortage, and inter-species harassment²⁴.

Evolutionary Advantages

The observations of aggressive behaviour in higher primates deserve consideration in the framework of an adaptive evolutionary view of aggressive behaviour. Although we cannot be sure of the ways in which aggressive behaviour has functioned adaptively during the course of evolution, the following possibilities deserve serious consideration: (1) increasing the means of defence; (2) providing access to

valued resources such as food, water and females in reproductive condition; (3) contributing to effective utilization of the habitat by distributing animals in relation to available resources—a spacing function of inter-group tension; (4) resolving serious disputes within the group; (5) providing a predictable social environment; (6) providing leadership for the group, particularly in dangerous circumstances; and (7) differential reproduction—it is compatible with present knowledge (though not proved) that relatively aggressive males are more likely to pass on their genes to subsequent generations than less aggressive males, because of the consort pairing at the peak of oestrus in which male aggressiveness plays some role.

Some of these factors may have given selective advantage to aggressive primates, if they could effectively regulate their aggressive behaviour. They have well defined cues that usually terminate aggressive sequences, and an elaborate repertoire of submissive behaviour. They have a stable and agreed dominance hierarchy that contributes to the predictability of the social environment, and they have clear, vivid sequences of aggression-submission-reassurance that have elements in common with the behaviour of man. Future research will profit from paying as much attention to the regulation and control of aggressive tendencies as to their sources and instigation.

If evolution is given serious consideration, it is difficult to overlook the likelihood that man has a vertebrate-mammalian-primate heritage predisposing him to aggressive behaviour. But what is known of the processes that govern the expression of such predispositions? To try to answer this question I shall consider the development of aggressive behaviour within the individual life cycle. Hormonal influences on brain organization early in life affect later aggressive and sexual behaviour²⁵⁻²⁷. Even brief treatment of newborn female rats with testosterone produces lifelong abolition of female sexual behaviour and a tendency to male patterns of aggressive behaviour. Pregnant rhesus macaques given large doses of testosterone during the second quarter of gestation produced female offspring which were abnormal²⁸ with some anatomical and behavioural characteristics of the male type. One of the well documented behavioural characteristics of this species is a sex difference



Fig. 1 An adult female chimpanzee approaches a highly dominant adult male which has been behaving aggressively in the presence of a premium food (bananas). She is reaching out in a manner which typically elicits a hug or pat; this is very similar to human patterns of greeting and reassurance. (Picture taken by Eric Hamburg.)

in the behaviour of infant monkeys, the males being more aggressive. This difference can be measured reliably in the first few months of life in the laboratory. Similar sex differences in aggressive behaviour have been observed in wild macaques and baboons.

Female infants of mothers given testosterone during pregnancy are more aggressive than normal females. They threaten other infant monkeys more often, initiate play more often, and engage in rough and tumble play more often than do normal females.

During growth and development, many influences can modify the expression of these aggressive patterns. For example, undue infant aggressiveness may be severely punished by larger and more powerful animals, leading to fearful inhibition later in life. In view of these possibilities for environmental influences during development, it is interesting that there is some tendency toward persistence of hyper-aggressive characteristics into adult life in females whose mothers are exposed to testosterone in pregnancy.

Human Development

Is there any evidence that testosterone has similar effects on the development of behaviour in humans? We have been able to study a few cases of girls who had been exposed to androgens *in utero*. At the Pediatric Endocrine Clinic at Johns Hopkins University, John Money has been conducting a long term programme of research, and a report has been published on twenty-two girls, mostly studied in late childhood and early adolescence³⁹. Those with striking anatomical abnormalities had undergone surgical treatment shortly after birth. By means of interviews and projective tests, information was obtained from each girl and from at least one parent in each of several behavioural categories. The research design undertook to control for observer bias. Results indicated that the early androgenized girls, in contrast with a control group, tended to be described by themselves and others as tomboys, to engage in outdoor sports requiring much energy and vigour, to prefer rough play, and to prefer toys ordinarily chosen by boys (such as guns). This initial study requires replication, even though replication will necessarily be difficult. This line of inquiry has led from basic research on the biology of sex differentiation to an important human problem. The results are at least compatible with the concept that testosterone is one of many

influences which shape the development of aggressive behaviour in the human species³⁰.

Does the exposure of foetal brain to testosterone have a distinctive influence on the differentiation of circuits that will later mediate aggressive responses? This seems likely in the light of current research on biochemical mechanisms of brain differentiation³¹. This does not, however, suggest that testosterone establishes fixed patterns of complex behaviour, unmodifiable by subsequent events. More likely, the hormone affects the brain in such a way as to facilitate the learning of aggressive patterns later in life. For any species, some patterns of behaviour are easy to learn, some fairly difficult, and some exceedingly difficult³². In general, patterns that have been valuable in species survival are easy to learn³³; and hormonal influences on brain development may have important mediating roles, especially where sex differentiation is concerned³⁴. One way this can occur is through differences in the development of attention, for attention is crucial in learning processes. A hormonal influence during a sensitive period of brain development may render a certain class of stimuli more interesting to the organism at a later stage. This line of inquiry is amenable to experimental analysis in primates.

Visual Stimuli

Do some types of visual input elicit more sustained attention from infant monkeys than others? In one experiment, monkeys were raised in a total isolation chamber for the first nine months of life, during which they were exposed to various types of visual input from coloured slides³⁵. These slides depicted monkeys in various activities and also depicted various non-monkey stimuli. Monkey pictures elicited much more interest than non-monkey pictures, and moreover, pictures of monkeys showing a species-typical threat expression were especially potent in eliciting behavioural responses. Between two and a half and four months of age, threat pictures yielded a particularly high frequency of disturbance. Similar results were obtained with films in the same experimental design (personal communication from G. P. Sackett). As with most studies in this new area of scientific inquiry, this calls for replication. It suggests that the infant rhesus has a built-in, special responsiveness to the threatening facial expression which is characteristic of its species. Given a responsiveness of this sort, it is not difficult to imagine how the infant in the natural environment would learn a great deal about threat and attack behaviour. Once the infant monkey's attention was powerfully drawn to threat stimuli, he would rapidly learn the conditions in which threat and attack patterns were likely to occur, and the actions likely to terminate aggressive sequences.

This brings me to a consideration of observational learning in primate adaptation³⁶. Traditionally, discussions of aggressive behaviour have associated unlearned responses with non-human creatures and learned responses with humans. Curiously, this linkage has persisted in spite of the enormous research literature on learning in non-human animals.

One of the most interesting findings of the primate field studies in the past decade has been the recurring theme of observational learning in a social context³⁷. In many species and diverse habitats, the following sequence has been described: close observation of one animal by another; imitation by the observing animal of the behaviour of the observed animal; practice of the observed behaviour, often for some hours after its occurrence, especially in the play group of young animals. This sequence of observation, imitation, and practice has been described in relation to several adaptive situations: food gathering, tool-using and tool-making (in chimpanzees), nest building, infant care, copulation and aggressive interactions. In essence, the



Fig. 2 Two adult male chimpanzees engaged in an intense, prolonged session of mutual grooming, immediately after aggressive interactions over premium food (bananas). (Picture taken by Eric Hamburg.)

young have access to virtually the full range of adult behaviour, and take advantage of it to learn patterns of behaviour that have been effective in adaptation. The motivation for such observational learning seems to be well established in the young primate. Observational learning in monkeys has also been studied experimentally. It has been demonstrated that one monkey readily learns from watching another in food choice situations. Moreover, the observing animal learns from incorrect as well as correct responses of the animal he is watching³⁸. In other words, he learns from the consequences of the operating animal's behaviour.

Sex Differences

In this context, our field observations are consistent with two concepts: (1) aggressive patterns are learned early in life through observation-imitation-practice sequences; (2) there is a sex difference in the attractiveness of some aggressive patterns from infancy onward. In baboons and chimpanzees, aggressive play is prominent during growth and development, with males spending considerably more time than females in such activities. Chimpanzee males (and also gorillas) make elaborate aggressive displays in adult life, and the young males, even in infancy, show more interest in such behaviour than do females. Young males are keen observers of older males in such matters.

This line of inquiry is important for an understanding of the influence of the social environment on the development of aggressive behaviour. It provides another link between evolutionary and developmental views of the organism. Lately, developmental psychologists have been calling attention to the importance of observational learning in young children³⁹. The child between one and two years of age is a devoted watcher. At this age, observation and imitation may well provide his principal mode of learning.

A remarkable set of findings has highlighted the susceptibility of children to learn aggressive patterns by viewing models which act aggressively. For example, in one experiment pre-school children were exposed to an aggressive model attacking a target object for ten minutes in a laboratory situation, while a control group experienced the same situation without an aggressive model. When the children were tested in the same situation six months later, the former were much more aggressive towards the target object than the latter—that is, a ten-minute exposure enhanced physical aggressiveness in the same situation six months later. This experiment is one of a series carried out by Bandura and his colleagues at Stanford during the past few years^{40,41}, showing vividly how children can learn aggressive patterns by watching film or television and enact these patterns in their later play. He has shown that pre-school, middle class children tend to imitate the aggressive behaviour of adults whether they observe it in people in their presence or depicted on film; moreover, the effect is obtained even with cartoon films. In short, aggressive patterns are imitated whether the dramatic presentation is realistic or fantasy-like. Imitation is increased if the aggressive model is rewarded in the drama, decreased if the aggressive model is punished. In general, these imitative acts are displayed spontaneously in the play of these young children, sometimes in remarkable fidelity to the model's original behaviour. But recent work goes beyond this, showing that the children are capable of performing imitatively many more aggressive acts than they show spontaneously—provided they are given an incentive for doing so. Thus, there is a tendency to restrain expression of aggressive patterns in spontaneous play; this is particularly true for girls and for patterns in which the model has been punished. But the patterns have been learned, and their performance can readily be elicited by a suitable incentive. It is worth emphasizing that these studies rely on direct observation of the child's behaviour in the experimental setting.

Thus, it seems that biological predispositions to learning aggressive patterns and exposure to specific social learning situations may interact to produce great individual differences in aggressiveness during later life^{42,43}. In analysing such problems scientifically, the effective conjunction of biological and psychosocial disciplines—so far rarely achieved—holds much promise for future understanding. It hardly seems necessary to point out the aggressive tendencies of the human species today. Whatever adaptive functions such behaviour may have served in man's evolutionary past, there is serious question about its utility in contemporary society⁴⁴. The risks inherent in such behaviour have been greatly amplified within our lifetime. Yet these problems at present attract only a modest amount of attention in the scientific community. It is difficult to imagine a more important area for research in the future.

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Role of the Research Councils

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Last December a working party was set up by the Council for Scientific Policy to review the role and function of the research councils in Britain. At the invitation of the Editor of *Nature*, the secretary of the Medical Research Council has defined the problems facing the councils, and drawn some personal conclusions about future needs.

BRITAIN invests considerable sums in research, and of this rather more than £100 million are spent through the research councils. Most people would agree that in spite of the importance of the educational and cultural aspects of scientific knowledge, sums of this order are provided because the nation expects a practical return for its investment, at least in the long term. The question that is being asked is: what sort of organization of this area of science is likely to provide the best return for money? This objective has to be considered in the light of the nature of science. Science stems from original ideas; original ideas are the products of independent minds and, by definition, nobody can order originality or predict its outcome. This is important but it must not be forgotten that there is much hard and often tedious work to be done as well.

The aim therefore is to use science, with full regard to its nature, to achieve the practical ends which Britain requires. To do this, the administration must carry out certain tasks. These can be summarized, though inevitably with oversimplification. If research is to be done there must be a national capacity to do it and, unless the future is to be mortgaged, an important task is to maintain and develop that capacity; this is a capital investment. This task demands, for example, the formulation of postgraduate training award policies, the support of particular disciplines, the encouragement of interactions between fields and the maintenance of a broad foundation on which the at present unforeseen needs and opportunities of the future can build. The next task is to define objectives. Usually in research there is no straight course to the mark,

and most of the course is likely to be unknown at the start; this does not, however, make it any the less important to know where the mark is. The determination of the broad objectives and priorities is clearly a matter in which the government has a considerable interest. The problem is largely one of relating, on the one hand, estimates of the benefits hoped for and, on the other, estimates of the chances of success, likely time scale, etc., in the existing state of knowledge. A further task is to formulate and execute the policies needed to achieve the objectives with regard to the nature of science. The final task I wish to mention is that of communicating results of practical importance to users and providing advice on the interpretation of results.

Interrelations of Science

The concepts of the scientific capacity of the country and its development, and the formulation of scientific policies with objectives in view, relate to what are sometimes called the "horizontal" and "vertical" elements in the structure of science.

The horizontal elements relate to the development of a coherent body of knowledge within disciplines or schools of thought. Individuals, universities and learned societies are basically responsible for these. Nonetheless, the research councils have a significant role to play, perhaps particularly in relation to the balance between disciplines, through their support of university research and training complemented by the use of their own establishments. It is the research councils collectively which are responsible for the provision from public funds of selective support for science in the universities. The policies of the councils, which need to be and increasingly are being coordinated, can play a significant role in the pattern of research. The councils can encourage particular disciplines; some, for example dentistry or nuclear physics, are within the responsibility of a single council; others, such as psychology or biochemistry, need coordinated action from more than one council. The need for horizontal integration is widely accepted and the research councils are very conscious of their joint responsibilities for coordination.

The vertical element in the structure—the development of knowledge towards practical objectives—is the one which appears to be the more controversial. Thus one type of solution put forward to solve these problems of organization

is to divide research into "pure" and "applied", and to separate them administratively. Funds for pure research would then have to be justified as a whole but in isolation from the impact of its practical application; they might be justified as support for higher education. This is obviously a possible line of argument and I think it important to discuss the distinctions between pure and applied research, particularly in relation to medical research, and to ask whether a useful division is possible. In the medical field most people think of clinical research as the applied end. To a great extent this is true, but much clinical research is long term in its aims and fundamental in its results. Equally, much laboratory work in medical research is short term and has immediate practical implications. Clinical research is normally carried out by individuals with small teams and it is carried out particularly in teaching hospitals in universities. The same is true of laboratory research in the medical field; the great exception is, of course, the major and important work of industry, particularly in therapeutic substances. Organization in small teams is also found in the so called applied parts of fields other than medical research. It applies equally to much work in chemistry and in engineering and agricultural faculties in universities, to much of the work of the Science Research Council (SRC), and to much done in institutes of the Agricultural Research Council (ARC). Research teams in many fields may undertake some projects of a long term nature and others intended to solve short term problems. It is sometimes only when the results are obtained that it is possible to say whether a project is long term or short term in its implications.

There are, of course, areas of research, for example aircraft, which because of their special demands must be treated separately. However, scientific policies designed to advance knowledge towards general answers to particular practical objectives must include training and research developments over the whole range of relevant studies, pure and applied. Such policies will clearly be most effectively formulated and executed by a single body when this is practicable. Furthermore, since any particular scientific project has its place on both horizontal and vertical dimensions, the two cannot logically be separated. The fitting of general answers to situations particular in respect of place, time or political need is by its nature a separate problem.

Medical Research Council

I would like now to describe something of the role and operation of the Medical Research Council with a view to illustrating some of these points. The council's purpose is to advance knowledge which will improve the health and capability, both physical and mental, of individual members of the population; this involves not only the prevention of disease and, should prevention fail, its treatment, but also a positive approach to improving physical and mental wellbeing. Such knowledge is of value not only to medical men but to many others including, for example, planners, educationists, social workers and industrialists; it is of value to the various health departments and to many other activities of government. The research is carried out whenever possible through the agency of a university but this is complemented by the direct employment of staff. In order to achieve results, to play its part in developing Britain's scientific capacity and to make the best use of individual originality, the council has three broad policies. These are: to support in its particular field work which is of outstanding quality regardless of subject; to provide at least a minimum of support over this whole area of science, and to support and undertake research in accordance with scientific policies designed to encourage a balanced development of knowledge from fundamental research towards practical objectives, particularly in priority fields. (For further information on council policies see MRC annual reports for 1968-69, 1969-70 and 1970-71, yet to be published.)

The council keeps the whole of its field under review and

decides at regular intervals which areas, organized towards broad practical objectives, should be reviewed in greater depth or given priority. It also determines reviews and priorities in relation to disciplines which need special encouragement. In determining priorities and areas for review the council hears the views of the health departments, whose assessors attend council meetings, of users, since the clinical members of the council are all practising members of the medical profession as well as investigators, and of other government departments and users through various committees. In general, the statement of objectives is not contentious; we all know we want to prevent or cure cancer or heart disease or mental disorder. The problem is not to state the objective but to find the means to reach it. To formulate policies to work towards such objectives, all approaches must be taken into account. This can be illustrated by examples.

Recently, the council published a report on radiobiology, which deals with radiation hazards and radiotherapy (MRC annual report 1968-69, page 11), and on biochemical research in psychiatry¹. Summaries of other reports, three on drug dependence and one on applied physiology, will be published shortly. Each illustrates the essential need to include a broad range of scientific coverage in such studies. The radiobiology report advises, among other things, that the most practical approach to the immediate applied problem of the quantification of risks from levels of radiation near normal background "should increasingly proceed by analytical methods defining the process of damage and recovery", that is, at sub-cellular levels. This field is one of long standing collaboration between the ARC, which is responsible for food, and the MRC; there is a joint standing committee on monitoring fall-out. The reports on drug dependence range from the chemistry involved in tracing breakdown products, through pharmacology to social problems. The Social Science Research Council (SSRC) is represented on the joint working party set up to implement the recommendations. Applied physiology, a partner to applied psychology on which there was a report a few years ago, is concerned with man in his environment and his efficiency at work. This field is one which used to be under the aegis of a joint MRC/DSIR (Department of Scientific and Industrial Research) board and at present requires consultation with both SRC and SSRC. A joint report will soon be published on the work of all five councils concerned with pollution. The MRC has also recently undertaken studies of certain disciplines, in particular, clinical pharmacology, obstetrics and gynaecology, mycology and dermatology.

The report on biochemical research in psychiatry¹ is a useful example to illustrate in a little more detail how the council can operate towards a specific objective. As is well known, this is an extremely difficult field of research. In spite of the large growth in knowledge of the mechanisms and behaviour of nervous systems, it is still very inadequate. Furthermore, the technical problems of relating the available knowledge of the nervous system with changes which occur in mental disorder in the intact human are immense. Of particular importance is the fact that there is a great shortage of people with the appropriate training and interests. Simply supporting psychiatrists to make observations on sick patients in mental hospitals will not solve the problem. There is clinical research to be done, but the committee recommends more work of a fundamental nature and in particular more work in what it describes as "bridging disciplines". It also notes the shortage of psychiatrists who have sufficient training in methods of investigation. The council has accepted the recommendations and is acting on them. It can support work in which there is interest and activity by awarding grants in response to applications; this applies in various fields of neurobiology. Sometimes an active initiative or special facilities are required. The council is tackling the bridging area by re-forming an old establishment and constituting a new one, each with both clinical and various laboratory studies. The MRC can also attempt to improve the shortage of trained personnel. It can give training

awards and fellowships, make staff appointments and give grant aid for assistantships; more important, it can, with its long term programme grants and groups, help to strengthen the research sides of departments of psychiatry. The moral of this example, and it is typical of many, is that the best approach involves a combination of varying aspects of laboratory and clinical research, together with a need to encourage a discipline and to increase training.

This example raises other relevant issues. In the formulation of the policy, in its implementation and in the staffing of the multidisciplinary establishments, the council has to be deeply involved in many aspects of fundamental neurobiology. However, for good reason the SRC is also interested in neurobiology; the two councils therefore have a joint committee on the subject. The neurobiological approach to mental health considered in this particular report is only one; there is also a great range of very important problems on the interface of psychiatry and sociology, and in research units concerned with social aspects of psychiatry the MRC has received help from the SSRC.

There are a number of disciplines which have to be an integral part of the work of more than one council if each is to fulfil its responsibilities; biochemistry, physiology and psychology are examples each involving three or four councils. There are also areas in which more than one council may be involved in policies designed to advance knowledge towards a broad practical objective. Specific examples have already been mentioned; more general examples can be given. The MRC has sections dealing with social medicine and environmental medicine which must have close connexions with respectively the SSRC and the Natural Environment Research Council (NERC). The MRC is concerned with nutrition, the ARC with food; there is a joint committee covering both fields. There does need to be very close coordination between councils; it has been improved considerably and is being further developed.

The principal user, in a practical sense, of the knowledge covered by the MRC is the medical profession; communication with the medical profession is largely through scientific publications, scientific meetings and so on. The council has, of course, close relations with the various health departments. There are important aspects of these relationships which have no bearing on information and advice, and I shall not consider them here. Close liaison is maintained by mutual representation on committees, through officers and by formal communication. The same is true of a number of other government departments, and since these relationships are less obvious I will give some examples. The council advises the Overseas Development Administration of the Foreign Office and the funds for tropical medicine of ODA and MRC are distributed on advice from a board of the council. Similarly, work for the Army and Navy is dealt with by standing joint committees. Problems ranging from food additives to the trawler disasters have been investigated for the Ministry of Agriculture, Fisheries and Food, sometimes by joint committees. There has been constant communication with various departments on industrial conditions, environmental problems and so on, and with the Home Office on subjects such as drug dependence and delinquency.

Another general and important point arises out of the previous paragraph. The research councils have an advisory function, often to government but sometimes to other public bodies, and it can be of advantage to government, parliament and the public to be able to get a scientific view from a body which is not only independent but can be seen to be independent of the department responsible for the decision. Probably the major examples of this in the past have been the Medical Research Council's advice on problems of radiation and safety.

Some Conclusions

I should like now to make some conclusions which are essentially personal. I have suggested that administration in the areas of science under consideration is concerned with four broad

tasks: the development of the scientific capacity of the country, the definition of objectives, the formulation and execution of scientific policy and the communication of results and advice. Of these the first and third are matters of administration of science; the other two involve not only science but other areas of national life.

I think that there would be little disagreement that the two scientific tasks taken on their own would be best served by keeping the administration of the main core of pure and applied science as closely coordinated, and indeed integrated, as practicable. The attainment of practical ends could only be hindered if scientific policies could be neither formulated nor implemented effectively. As has been illustrated, such policies need to cover not only a wide range of research, both pure and applied, but also questions involving the development of disciplines. The practical need in administering these tasks is for a flexible structure which will enable the horizontal and vertical—and any other—dimensions, used to describe the pattern of science, to be considered together, and which can change with changing patterns of science and social and economic need.

The task of communicating results and advice in a way which will achieve changes in practice needs thought. It involves several elements. There is communication by means of journals, meetings and scientific or professional bodies; this is, or can be, an effective means in relation to medical practitioners, engineers, industrial scientists and so on. There is communication with government departments; to achieve this there is and must be a considerable network of committees and personal contacts because results in any particular area of science may have impacts in many parts of government. The most difficult task is probably to get information, in suitable form, to others such as farmers, small firms which do not employ professionals and even, sometimes, large firms in science-based industry in relation to less familiar problems, such as human factors in engineering design. Whether or not systems of communication, such as the existing advisory services of MAFF, should be set up and by whom they should be run are essentially political questions; scientists and administrators of science can only express a willingness to help.

The question of how to distribute finance and to determine priorities is undoubtedly crucial. This is a matter in which government departments, usually several in respect of any one council, have a genuine interest. Objectives are often easy to name and may in any case be first noted on the research side. It is usually more difficult to assess the feasibility and the effect that a particular use of research capacity will have on the progress of other research; these are matters of scientific judgment. The value of achieving the objective should be balanced against feasibility and the likely cost to other lines of work. All councils have day to day machinery for dealing with such matters; for example, assessors from departments sit on councils, officers are in constant contact at all levels and there are many joint committees. There may be a need to improve such machinery and there may be times when it does not function adequately and major differences remain unresolved. The final judgment and sanction do and must lie with government; it is for government to determine by whom the judgment should be made between the conflicting interests of professional and industrial users, of different government departments and of the general development of the country's scientific capacity.

There are problems which need consideration: how to improve coordination in the administration of science; how to improve communication with users; how to improve the responsiveness of science to national needs. When these problems are discussed one thing must be remembered: the hard task is not in recognizing a need or in communicating an answer, it is in finding the answer to meet the need.

¹ *Biochemical Research in Psychiatry: Survey and Proposals* (HMSO, London, 1970).

Antimatter in the Universe

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The models of nucleosynthesis discussed in this article could give clues to the possible existence of aggregations of antimatter in the universe.

THE symmetry between the properties of particles and antiparticles has been established with great accuracy. In particular, it has been shown from quantum electrodynamics that the electromagnetic interactions responsible for atomic properties are the same for matter and antimatter, so that it is impossible to distinguish these two forms of substance by optical measurements. It is very natural, therefore, to ask whether our universe is made only of matter or by both matter and antimatter.

From the point of view of astrophysics, two basic questions have to be answered if the latter possibility is to be considered seriously. The first is commonly stated as "Where does antimatter come from?" It is a difficult question because it obviously amounts to asking for a theory of the origin of matter itself. On the other hand, there is very good evidence, particularly from γ -ray observations¹, that at least the larger part of our galaxy is made chiefly of ordinary matter. The second question is therefore: "How can matter and antimatter be organized in units as big as a galaxy?"

According to the conventional hot big-bang model of the universe there was a small excess (~ 1 in 10^9) of baryons over antibaryons in the early stages of evolution, when the thermal energy kT exceeded the rest energy $m_p c^2$ of a baryon. It is to this excess that we owe the present existence of matter in the universe, for as the thermal energy dropped below mc^2 the baryons and antibaryons annihilated one another, leaving just the excess baryons intact. It has occurred to many people that it would be attractive to do away with the small excess of baryons at the early stages, if only a method could be found for separating matter from antimatter on a suitable scale before the annihilation set in. (Extensive references are given in the papers of Steigman¹ and Harrison².)

So far no satisfactory separation mechanism has been discovered. In view of the importance of the question, however, I believe that strenuous efforts should be made to find such a mechanism or to prove conclusively that none is possible. My aim in this article is to draw attention to a new possibility for a separation mechanism and to discuss some of its implications. The mechanism is based on the possibility that the thermodynamic properties of thermal radiation can be significantly modified by strong interactions³⁻⁵. It is an open question whether the Gibbs potential Ω for radiation, which is defined by

$$e^{-\Omega/kT} = \text{Trace } e^{-H/kT} \quad (1)$$

(where H is the total Hamiltonian including strong interactions), has no singularity as a function of kT for large values of the temperature (see, for example, ref. 6). Such a singularity might be the indication of a phase transition. To be more specific, it is important to see whether there is some aspect of the strong interaction which gives an indication that such a phase transition exists and that its effect is to separate spatially nucleons from antinucleons.

One such model has been analysed in some detail. It starts from results of the mesonic theory of nuclear forces arising from the exchange of mesons between nucleons. The modern approach to this theory is to use dispersion relation techniques. From a fit of nucleon-nucleon scattering by the exchange of π , η , ω and ρ mesons, the nucleon-antinucleon scattering amplitude can be derived. There are bound states of nucleon-antinucleon pairs which can be identified with the mesons π , ρ , ω , η ⁷. Such a situation in which the same particles appear as bound states (or resonances) and as agents for the forces is a special case of the notion of "bootstrap" for particles.

Current theories of particles place on the same footing elementary particles and bound states, which should be taken into account when dealing with the thermodynamic properties of a system containing nucleons and antinucleons. An adequate formulation for the foundations of statistical mechanics has been proposed⁸ which can be summarized in a basic formula relating the Gibbs potential Ω to its value Ω_0 for free particles (including bound states) and to the collision matrix S

$$\Omega = \Omega_0 - \frac{kT}{2\pi i} \int dE e^{-E/kT} \frac{\partial}{\partial E} \text{Trace} [(\text{Log } S(E))_c e^{-\sum \mu_i N_i}] \quad (2)$$

E is the total energy parameter, and the index "c" means that only connected terms in the sense of quantum statistical mechanics should be taken into account. The values of N_i are additively conserved quantities (charge, baryonic number . . .) and the values of μ_i are the corresponding chemical potentials. A special case of formula (2) is the classical Beth-Uhlenbeck expression for the second virial coefficient b_2 of a gas in terms of the scattering phase shifts $\delta_l(E)$ describing the collisions between two particles of the gas

$$b_2 \simeq \frac{1}{\pi} \sum_l (2l+1) \int_0^\infty \frac{d\delta_l(E)}{dE} e^{-E/kT} dE \quad (3)$$

No term associated with bound states is written in equation (3) because, according to formula (2), it is already included in Ω_0 . The detailed justification of this point is an important feature of the Dashen-Ma-Bernstein approach.

Formula (3) shows the root of the effect of separation between nucleons and antinucleons: when bound states exist, some phase shifts in $N-\bar{N}$ scattering start from the value π at the threshold energy for collisions and go down to zero when the energy becomes large. This property is known

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as the Levinson theorem and is valid in the calculations of ref. 6. As a result, b_2 can be negative, just as it is for a repulsive interaction (which I shall call a statistical repulsion), although the nucleon-antinucleon interaction is chiefly attractive. (Similar effects are known in the theory of dissociation.)

Formula (2) is very convenient when dealing with elementary particles where the scattering amplitudes (the S-matrix) are known from experiment or from theoretical models whereas the Hamiltonian H is neither known nor useful. Using (2), one can investigate the behaviour of Ω as a function of the densities N and $\bar{N}$ of nucleons and antinucleons in thermal radiation. It turns out that the statistical repulsion is larger than any repulsion in the ordinary nuclear forces. On the other hand, one must take into account the effect of annihilation, which can be done by using a version of the Fermi statistical model. The result of the analysis is that, using conventional data, one finds a phase transition at a temperature kT of the order of 350 MeV. Above that temperature, nucleons and antinucleons tend to separate spatially from each other as a result of their statistical repulsion, so that the equilibrium local density of baryons $B = N - \bar{N}$ is found to be a non-zero quantity $\pm B_0(T)$, vanishing at the critical temperature. To be sure, it should be stressed that knowledge of strong interactions is too incomplete to prove that such a phase transition actually exists, but the analysis could be made more reliable by better data about nucleon-antinucleon scattering. I shall consider the existence of a phase transition of a type with an appropriate critical temperature as an Ansatz when I describe the evolution of matter in a big-bang model of the universe. Much of what I shall say would be valid in some other types of cosmologies, such as a version of Missner's mixmatter universe where previously independent systems of matter and antimatter come into contact.

Consider the simplest model of a universe which is initially filled up only with thermal radiation. Its expansion is described by a scale factor $R(t)$ which behaves approximately like $t^{-1/2}$ while the temperature varies practically like R^{-1} . For the early stages of the universe the effects of space curvature are negligible. It is known that the history of such a model can be divided into several periods, according to the content of thermal radiation: the hadronic ($kT \gtrsim 100$ MeV), leptonic ($kT \gtrsim 1$ MeV) and radiative ($kT > 3,000$ K) periods. Superimposed on that division, the evolution of baryons will lead us to consider three other periods: the separation ($kT > 350$ MeV), annihilation ($kT > 25$ KeV) and coalescence ($T > 3,000$ K) periods.

In the separation period, nucleons and antinucleons separate spatially as a consequence of their statistical repulsion. This is initially induced by fluctuations and one can compute the size d that individual condensations containing an excess of nucleons or antinucleons reach during the 10^{-5} s of that period. It is essentially controlled by diffusion so that $d \simeq (Dt)^{1/2}$. Taking for the diffusion coefficient $D \simeq c \times 1$ fermi and $t = 10^{-5}$ s gives $d \simeq 3 \times 10^{-4}$ cm. Since the baryonic density B is then of the order of the nuclear density, the total baryonic number of such a condensation is typically 10^{28} . Near the end of the separation, the universe is filled with an emulsion of nucleons and antinucleons with a typical size d .

Next comes the annihilation period. When T falls below the critical temperature, nucleons and antinucleons tend to annihilate. Disregarding secondary effects for the sake of brevity, the annihilation process is found to be controlled by diffusion so that the densities N and $\bar{N}$ of nucleons and antinucleons satisfy the equations:

$$\begin{aligned} \frac{\partial N}{\partial t} &= D \nabla^2 N - \alpha N \bar{N} \\ \frac{\partial \bar{N}}{\partial t} &= D \nabla^2 \bar{N} - \alpha N \bar{N} \end{aligned} \quad (4)$$

and for the baryonic density $B = N - \bar{N}$

$$\frac{\partial B}{\partial t} = D \nabla^2 B \quad (5)$$

From equation (5) one derives the evolution of the correlation function for B

$$\begin{aligned} \langle B(0, t), B(x, t) \rangle &= [4\pi D(t - t_0)]^{-3/2} \int \langle B(0, t_0) B(y, t_0) \rangle \\ &\exp \left\{ - \frac{(x - y)^2}{2D(t - t_0)} \right\} d^3y \end{aligned} \quad (6)$$

As a result, the size of the fluctuations remains equal to $\sqrt{Dt}$ and the average baryonic density at time t : $\langle B^2(0, t) \rangle^{1/2}$ is of the order of the fluctuation $(\Delta B^2(t_0))^{1/2}$ at time t_0 (10^{-5} s), within a region of size $(D(t)t)^{1/2}$, times a factor $(t/t_0)^{-3/2}$ coming from expansion. In practice, expansion leads also to the replacement of Dt by $\int D(t) dt$. When computing $(\Delta B^2)^{1/2}$, one should take into account the correlations induced into the system by its originating from an initially homogeneous radiation. At the end of this period a typical fraction 10^{-8} or more of the initial nucleons survives. They are still in the form of an emulsion with a typical size 10^5 cm, and a typical mass 10^{10} g, within a sphere of this radius. This is very far from a galactic mass and it is essential in this theory to find a mechanism which could lead to the coalescence of matter and antimatter into larger units.

During the coalescence period annihilation gives birth chiefly to pions and, through their decay, to high-energy photons, electrons, positrons and neutrinos. The transfer of momentum by the photons and electrons produces an annihilation pressure at the boundary between matter and antimatter. (The existence of this annihilation pressure was pointed out by H. Alfvén and O. Klein⁹.) This pressure is exerted over a distance λ_γ , the mean free path of high energy photons.

To find the behaviour of matter and antimatter which are in contact along a common boundary is a rather complicated problem in fluid mechanics. The effect of high energy photons (and leptons) is a dominant feature because these particles exert a very strong pressure and keep heating the system. Radiative pressure is dominant, so that the pressure due to heating tends to balance the annihilation pressure. It has been shown that, at least for two-dimensional configurations, only the linear and circular boundaries are stable and that other shapes of boundaries suffer deformations which tend to increase their radius of curvature, as long as it is smaller than λ_γ . The actual problem in cosmology is also difficult for geometrical reasons. Matter and antimatter are distributed in an emulsion and their boundary is most probably a very complicated Gordan surface. It can be shown, however, that if there is a systematic excess of photons entering one side, such as would be produced by a bulging of the boundary, there should be a rapid compensating motion of the boundary. This suggests very strongly that the annihilation pressure tends to keep the size d of the emulsion to the order of λ_γ when λ_γ increases by expansion. Irreversible thermodynamics also leads to the same conclusion.

During the annihilation period, $d \gg \lambda_\gamma$. The equality $d = \lambda_\gamma$ takes place for $kT \simeq 25$ keV. Afterwards, d remains of the order of λ_γ ; that is, it increases very rapidly, because annihilation provides enough energy for the corresponding coalescence of baryonic condensations into larger homogeneous units.

To follow the evolution of the density and of d as a function of time, it is necessary to evaluate the rate of annihilation v per unit of area of the boundary and per unit of time. In its full generality the problem is complicated, but simple results could be obtained in the following way. At the boundary between matter and antimatter, there is a region of overlap. In that region, the annihilation effect determines the mean free path of nucleons, so that their rate of annihilation is controlled by their thermal velocity. Because radiation pressure is dominant, no inhomogeneity in the local density

of matter can develop² so that matter and antimatter flow towards their common boundary at the thermal velocity of the protons v . As a result

$$v = Nv \quad (9)$$

This gives only a small relative amount of annihilation during the coalescence period. Nevertheless, this annihilation is sufficient to provide enough energy to keep the equality $d = \lambda_\gamma$ till the recombination period.

If one estimates the ratio η between the density of baryonic number and that of photons to be around 10^{-1} near the end of the separation period, one gets $10^{-12} < \eta < 10^{-8}$ at the time of recombination. Presumably the effect of coalescence stops then if only because the corresponding motion of the fluid would be supersonic.

The size of the emulsion and its density are found at that point to be $10^{21.5}$ cm and 10^{-20} g/cm³ so that the average mass of a matter region is typically 10^{11} M; that is, of the order of a galactic mass.

I may also mention the new form taken by the problem of nucleosynthesis in such models¹⁰. Nucleosynthesis takes place during the leptonic era, in the emulsion of matter and antimatter which has at that time a very large density, similar to the densities which have been considered by Wagoner, Fowler and Hoyle¹¹ for the case of bouncing massive stars. The chief features of these high density expanding models are that they produce a large abundance of helium (equal amount in mass of H and He) and a relatively large amount of heavy elements but a small amount of low mass elements. During the intense annihilation after nucleosynthesis, most nuclei disappear. Protons and antiprotons are partly regenerated by $H-\bar{H}$ e, $\bar{H}-\bar{H}$ e and $\bar{H}$ e-He collisions. The present

ignorance of the corresponding cross-sections does not yet enable us to calculate the resulting hydrogen/helium ratio.

Finally, such considerations suggest a revival of interest in the matter-antimatter model of quasars¹² and galactic nuclei. The arguments which were used to rule out this mechanism do not apply to the structures which evolve from the emulsion, that is, a compact mass of matter surrounded by antimatter or vice versa, in particular because they do not take into account the existence of magnetic effects.

To avoid misunderstanding, I stress that there are three aspects to the discussion of antimatter, namely the possibility of a phase transition in the black-body radiation, coalescence with galaxy formation and quasar models. None of them is necessarily associated with the other ones and the last two should be considered separately if one wants to discuss the experimental evidence for the existence or non-existence of antimatter.

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Effect of Oxidation on the Natural Remanent Magnetization of Titanomagnetite in Suboceanic Basalt

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The natural remanent magnetization of titanomagnetite is not destroyed by oxidation if the oxidation occurs below the original Curie temperature of the titanomagnetite.

THE palaeomagnetic memory of most igneous rocks resides in grains of magnetite and titanomagnetite which are chemically unstable. Under conditions of low temperature and high oxygen fugacity such as those which exist in rocks presently at the Earth's surface, titanomagnetite tends to oxidize to an equilibrium mineral assemblage of rutile and haematite, with intermediate formation of titanomaghemite, ilmenite, and iron-

rich titanomagnetite¹. The work reported here has been concerned with the magnetic changes resulting from the initial stages of such oxidation in submarine tholeiitic basalt. Of particular interest is the effect of submarine weathering on the magnetic remanence of submarine basalt, as well as the process by which basalt loses its remanence as it descends into the mantle.

Increases in the Curie temperature of basaltic rocks from high temperature oxidation have been observed both in the laboratory²⁻⁵ and in nature⁶. An increase in Curie temperatures from the weathering of submarine basalts on the sea floor has also been reported^{4,7}. The question of what happens to the natural remanence of basalt during oxidation, however, has received little attention. If the direction of the ambient magnetic field during oxidation is not parallel to the direction of the original natural remanence, it is important to know whether

the rock will retain a magnetic memory of the original magnetization or whether it will become completely remagnetized in the direction of the ambient field.

The question of the temperature at which oxidation occurs is crucial. The Curie temperature of the oxidized mineral is usually greater than that of the original mineral and so oxidation takes place generally in one of the following three ways: (1) The oxidation occurs at a temperature greater than the Curie temperature of the oxidized mineral. When it cools, the rock acquires thermoremanent magnetization (TRM). (2) The oxidation temperature is between the Curie temperatures of the original mineral and oxidized mineral. Chemical remanence (CRM) is then the only type of remanence formed; it is parallel to the field acting at the time of the oxidation, which may occur either during initial cooling or during subsequent reheating. (3) The oxidation temperature is below the Curie temperature of the original mineral. Remanence of two types may result: (a) the original TRM may persist through the changes in lattice structure accompanying oxidation, or (b) new CRM may be produced in the ferromagnetic minerals formed by the oxidation, and this new CRM is controlled by the ambient field at the time of oxidation.

Our work was undertaken to determine whether any memory of the original NRM persists through the introduction of cation vacancies and changes in the crystal lattice during oxidation. We were also interested in the intensity of the CRM produced by oxidation, and its spectrum of blocking temperatures.

The samples used included six specimens from a suboceanic basalt sill drilled in the eastern Pacific near Guadalupe Island², two specimens from a basalt pillow dredged 20 km from the axis of the Juan de Fuca Ridge⁸, and three specimens of pillows from the central rift valley of the Mid-Indian Ocean Ridge⁸. The opaque mineral in the pillow basalt was identified as titanium-rich titanomagnetite on the basis of its brownish colour, optical anisotropy, and low Curie temperatures, most of which were from 175° to 200° C. A Curie temperature of 325° C was measured in specimen 12-2, taken 2 cm below the surface of the Juan de Fuca pillow. Such a high Curie temperature is typical of the crusts of pillow basalts^{9,10}. This same pillow fragment also has a weathered rim and the opaque grains in specimen 12-2 have a somewhat silvery reflectance that suggests partial oxidation. Specimens of the submarine sill also had high Curie temperatures and contained an oxidized magnetic mineral identified as titanomaghemite on the basis of its silvery reflectance and lattice spacing (cubic cell edge of 8.383 Å).

Experimental Procedure

The objective of the experiment was to observe changes in the NRM of the samples when they were heated in air and simultaneously to observe the growth of new CRM. We used an astatic magnetometer, which consisted of a pair of north-south astatic magnets suspended 7 cm above the sample, a rotating sample stage with a nonmagnetic furnace, and three pairs of Helmholtz coils used both to cancel the Earth's field and to produce a known east-west magnetic field H (Fig. 1). The sample stage could be rotated about a vertical axis, so that any component of magnetization in the horizontal plane could be determined with a standard light beam system.

The key element in the design of these experiments was the alignment of the samples so that the NRM was exactly perpendicular to the applied field, H , during the heating. Because the resulting CRM and NRM vectors were orthogonal, their changes in intensity could be determined independently. Three steps were taken to ensure orthogonality during the experiment. First, samples were partially demagnetized at 100° C to ensure that they possessed only unidirectional NRM. The second step was to align carefully the astatic magnets so that they were exactly perpendicular to H . The third step was to use the condition of zero deflection of the magnetometer to

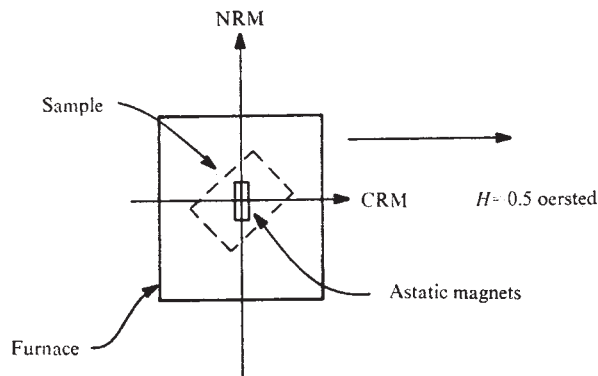


Fig. 1 Plan view of the experiment. During heating, the NRM of the sample was perpendicular to the applied field, H , and parallel to the astatic magnets. The field at the centre of the sample from the astatic magnets had an intensity of 0.01 oersted and a direction opposite to that of the NRM. New CRM due to oxidation of the titanomagnetite is assumed to form in a direction parallel or antiparallel to that of H .

determine the exact orientation of the stage needed to place the NRM of the sample parallel to the astatic magnets and thus perpendicular to the applied field, H , during heating. After the experiments were completed, the remanent magnetization acquired on cooling in the east-west applied field was measured with a spinner magnetometer as a check on the initial alignment of the NRM in the astatic magnetometer. In all cases, the final TRM was perpendicular to the initial NRM within several degrees.

A magnetic field H was maintained perpendicular to the NRM at all times during the course of the experiments except for brief intervals of 1-2 min, when it was switched off to permit measurement of the NRM and CRM. The intensity of H was adjusted to approximate to the field that existed at the sample sites when the lavas originally solidified, the assumption being that the field was that of an axial dipole with a moment of 8×10^{25} G cm³.

After partial demagnetization at 100° C, the sample was held at a constant temperature of 250° C for a period of half an hour, during which the NRM and CRM were measured at 10 min intervals. The temperature was then rapidly raised about 50° C and a new set of measurements taken at 10 min intervals was made at constant temperature. Increases in the two orthogonal components of remanent magnetization which occurred during the intervals of constant temperature are due to chemical changes occurring at that temperature.

Experimental Results

A typical run on pillow basalt is shown in Fig. 2. On heating to 250° C almost all of the NRM was destroyed and no CRM was produced, indicating that destruction of NRM was by simple heating to the Curie temperature rather than by chemical change. Repeat measurements slightly below the blocking temperature disclosed no evidence of viscous demagnetization. Noticeable chemical change began with further heating, and reached a maximum of about 400° C. A large fraction of the CRM grown at any temperature was destroyed when the sample was heated through the next 50° C, indicating that the Curie temperatures and blocking temperatures of the new CRM were only slightly greater than the temperature at which the CRM had been created. Above 500° C the rate of production of CRM was small, presumably because by then the oxidation reaction had gone to completion. The maximum Curie temperature of the CRM was about 580° C, essentially that of magnetite. The final step in the experiment was to cool the oxidized rock in the same field, H , to produce the TRM curve shown in Fig. 2. The blocking temperatures of the oxidized rock were in general greatly increased and the intensity of the TRM was several times that of the NRM, which presum-

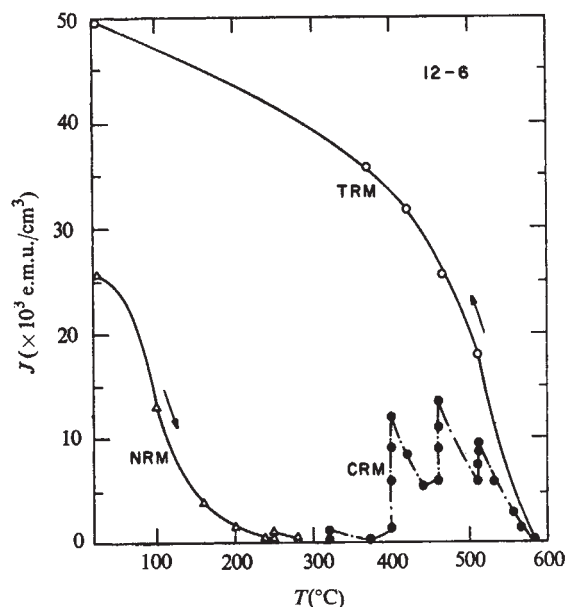


Fig. 2 Heating experiment in air on a sample in which oxidation occurs at a temperature above the Curie temperature of the original titanomagnetite. Δ , Changes in NRM; $\bullet$, CRM during oxidation; $\circ$, acquisition of TRM by the oxidized rock. The temperature was held constant for 30 min at 250°, 320°, 400°, 460°, and 510° C with readings every 10 min. H was 0.50 oersted. Specimen 12-6 was taken 6 cm below the glassy surface of basalt pillow 12 from the Juan de Fuca Ridge⁶

ably was chiefly thermoremanence acquired in a field comparable with H . This experiment exemplifies the case (2) in which the Curie temperature of the original ferromagnetic mineral is very low and the oxidation occurs above that temperature.

In other samples with higher initial Curie temperatures, oxidation corresponding to case (3) began before the Curie temperature of the NRM was reached (Fig. 3). The heating experiments on these samples establishes clearly that the NRM was not destroyed by the oxidation but rather that it increased in intensity during isothermal oxidation. The increase was probably due to an increase in spontaneous magnetization of domains with walls pinned by energy barriers that were not destroyed by the oxidation. The increase in NRM occurred over a slightly lower temperature range than the growth of CRM, the maximum increase in NRM occurring at 300° C. As was true of the CRM, part of the NRM which survived oxidation was destroyed on being heated several tens of degrees above the temperature at which the oxidation had occurred.

Oxidation first became measurable in these samples at temperatures between 200° and 300° C. Oxidation was favoured by open rock textures through which diffusion of air was more rapid and possibly by the presence of hydrated clay minerals which, when heated, produced water vapour that dissociated to produce oxygen. The effect of increasing the temperature was to increase the rate of oxidation. Where this occurred at temperatures above the Curie temperature of the original titanomagnetite (case (2)), CRM was produced parallel to the applied field. This magnetization was formed as the blocking temperature of the oxidizing titanomagnetite rose above the ambient temperature.

Where measurable oxidation began below the Curie temperature of the original titanomagnetite (case (3)), the NRM was changed by the rather complex sequence of events seen in Fig. 3. Two competing processes are evident: heating, which tended to destroy the NRM as the temperature rose through the initial range of blocking temperatures; and oxidation, which tended to increase the NRM by increasing the spontaneous magnetization and to preserve the NRM by raising the blocking temperatures. The NRM was not destroyed evidently because

the energy barriers that pin domain walls persisted through the oxidation. CRM was formed in domains heated above their blocking temperatures during a temperature increment as the result of partial oxidation which raised the blocking temperature above the ambient temperature.

What do these experiments tell us about the magnetic changes to be expected in rocks as they undergo incipient oxidation by natural processes? Theoretically CRM can be formed by either of two processes. If the ambient temperature during oxidation is higher than the initial blocking temperature of the magnetic material, a large amount of CRM is produced as the blocking temperatures rise above the ambient temperature (specimen 12-6). This process is probably not common in nature, however, where oxidation due to weathering takes place at a temperature far below the blocking temperature of titanomagnetite. Moreover, oxidation due to low grade metamorphism that occurs as rocks are slowly heated during burial is a gradual process which probably begins before the Curie temperature of titanomagnetite is reached. Initial oxidation during weathering or burial thus occurs generally below the Curie temperature, where CRM is formed as new energy barriers are created which pin domain walls. The intensity of the CRM created in this way, however, can be no larger than about a fortieth of the value of the induced magnetization. We conclude therefore that most of the natural remanence in partially oxidized basalt is original remanence that has retained its direction through the oxidation. It is not new CRM formed during the chemical alteration.

The role of heating in these experiments was to increase reaction rates to measurable levels. There are undoubtedly differences between the magnetic effect of oxidation in the laboratory at a temperature of 300° C for several hours and oxidation on the sea floor at a temperature of 0° C for millions of years. As evidence of this difference, the intensity of magnetization of submarine basalt has been observed to decrease with increased weathering⁷, rather than to increase as in our experiments. Yet our conclusion that titanomagnetite retains a memory of its original magnetization after oxidation offers an explanation for the existence of coherent marine magnetic anomalies produced by parts of the sea floor that are at least 70 m.y. old and which are now partially oxidized. The present results do not offer a ready explanation for the sudden disappearance of the magnetic anomalies at the oceanic trenches,

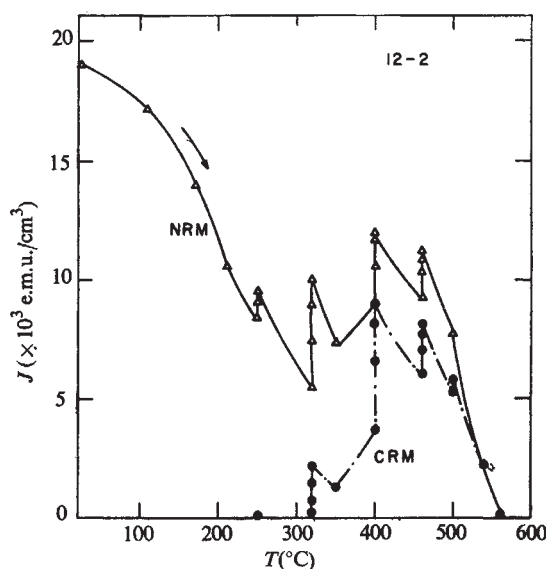


Fig. 3 Heating experiment in air on a sample in which oxidation began at a temperature below the Curie temperature of the original magnetic mineral. Other symbols as in Fig. 2. Specimen 12-2 is from the same pillow as the specimen shown in Fig. 2, but was taken 2 cm below the glassy surface of the pillow.

which probably reflects the complete destruction of the cubic magnetic minerals due to an acceleration of metamorphic reaction rates during burial and tectonic deformation.

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Polywater and Polypollutants

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Some careful experiments and a close look at the literature of polywater give no reason to believe that any new phenomenon has been discovered. Contamination could account for the observed effects.

THE concept of an entirely new species of water (known variously as polywater, water II, or anomalous water) owes its publicity principally to Derjaguin¹ and his co-workers. Their conclusions have been based on their own experimental work on the physical properties of water condensates in fine glass capillaries, and also on the earlier indicative observations by independent workers (for example, ref. 2). Derjaguin¹ in his latest article has reviewed his experimental work and confirmed that he still strongly believes in this new water species which, he claims, has abnormally high values for the molecular weight, viscosity (ten to twenty times greater), refractive index (1.48), boiling point (above 150° C) and anomalous density-temperature characteristics below room temperature. It has long been known that surface forces can greatly modify the properties of adjoining water layers. Derjaguin himself could be said to be the principal pioneer of this work during the past 20 years or so, for which he has justifiably obtained universal recognition. It must be stressed, however, that his water II phenomenon is of a completely different order from this, for he claims that water II can exist independently of the surface from which it has been catalytically formed. He claims that it does not lose its anomalous properties unless vaporized above 700° C. In the early days of these investigations he discussed the problem with J. D. Bernal (early 1968) and of his difficulty in getting the work recognized. Bernal's continued ill-health prohibited him from doing any really active research himself on the topic, and he left it to us to carry out the investigations under his general guidance.

If Derjaguin's chief contentions are correct, the problems facing the physical chemist are prodigious. He must interpret

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water II in terms of a completely new type of hydrogen bond which nevertheless can be easily formed during the condensation of slightly undersaturated vapours on to the appropriate surface. He must also explain why ordinary water—less stable than water II—is the predominant form in nature. Not surprisingly, Derjaguin found it difficult to convince other scientists, and a man of lesser reputation would have surely failed to do so. However, his continued assertions led many groups in the western world (notably in Britain and the United States) to follow up his work. These groups produced results which seemed to both confirm and contradict his findings. Furthermore, several theoreticians (for example, refs. 3–9) actively pursued the problem of determining a suitable quantum mechanical picture for the new bonding required to explain the anomalous properties; needless to say, some of these models were highly speculative¹⁰.

The evidence against the concept of water II has mostly accumulated very recently. Briefly, the contention is that the effects of even small amounts of certain impurities incorporated in the minute (say 10⁻¹⁰ to 10⁻⁵ g) samples have been underestimated. Derjaguin's defence of water II may seem impressive, but we believe that his account gives, however unintentionally, an incomplete presentation of the facts, and in view of its possible wide influence we feel we must state clearly our findings. We therefore wish to explain how our work and of others have led us to reject water II as a real phenomenon, and to examine more critically some of the claims of Derjaguin and his supporters.

The Phenomenon

Our investigations have been in progress for the past 2.5 years and we have used all known methods of production as well as introducing many modes of our own. Fuller details of this work are being prepared for publication later. (We have too much material to be included here, but some of our results have been mentioned by Hasted¹¹.) The degrees of anomaly in our samples were determined chiefly from observations on (i) the phase separation effects¹, (ii) the gel-like residue obtained after evaporation, and (iii) the shape of the density-temperature curve obtained after certain corrections had been made for the effect of the narrow capillary bore. The method of production was continually refined so

as to eliminate as many sources of contamination as possible. For example, before using a water pump (rather than a rotary pump) method, the residues obtained showed a higher carbon content as revealed by electron microprobe analysis and advanced mass spectrometric techniques. It was generally established that the degree of anomaly usually diminished as the apparatus and techniques were refined. We have now produced samples which are indistinguishable from ordinary pure water.

As a result of our studies and with some knowledge of the experiments of other workers, we have concluded that the most common sources of contamination in the general production of anomalous water are as follows (not in order). (1) Gross salt contamination, by surface migration and/or vapour transport, when salt solution (for example, K_2SO_4 or $NaCl$) methods of production are used. (2) Capillary residues (for example, nitrogenous or siliceous) resulting from the capillary "cleaning" process. (3) Leaching out of the intrinsic glass components (silicon, sodium and so on) from the capillary walls. (4) Surface migration and vapour transport of contaminants inherent in the apparatus; for example, (i) greases from joints, "O"-rings, insufficiently cleaned glassware, and sample handling; (ii) residues arising from chemical cleaning of the apparatus (as in 2); (iii) back-streaming of rotary pump oil vapours even with the use of cold traps; (iv) resident apparatus gases such as carbon dioxide; (v) gaseous or organic impurities located at apparatus occlusions, dust particles and so on.

Several groups have used the salt solution method, and their results must therefore be immediately discounted. Of those remaining, we wonder which apparatus could genuinely be claimed to be free of even the grease sources mentioned in (4i). The leaching products (source 3) are, of course, mostly unavoidable, and this, we believe, is why so much attention has been directed to identifying silicon as the principle impurity in anomalous water¹²⁻¹⁵. These contentions, however, of which most are based on little more than circumstantial evidence, are in complete contradiction with the facts. In the literature¹⁶⁻²⁰ and in our own work, impurity tests (most of which involve electron microprobe techniques) clearly show, with one exception¹⁴, that silicon is either absent or present in amounts that are minute compared with other "offending elements". Yet publications persist in alluding to silicon. We stress this point because we believe it partly explains the long time taken to recognize water II as an impurity effect—people have been suspecting the wrong impurities. In the final analysis, the real offending impurities are those of source (4)—particularly the organic impurities emanating from apparatus occlusions and even the sample handling in some cases²⁰. (It is worth noting that silicon vapours arising from the vacuum pump oil could also be present.) This is in keeping with the known facts on impurity tests (refs. 16-20 and our own work) in which organic substances (lipids and phospholipids) and the elements carbon, sodium, potassium, chlorine, sulphur (in rough order) notably appear in varying degrees of prominence. Even among the work supporting water II, Derjaguin's samples are recognized to contain organic impurities (see later) and Lippincott *et al.*⁴ admit to a small percentage of sodium and an unknown amount of carbon (see also other comments^{19,22} on the work of Lippincott *et al.*).

For and Against Polywater

It has been well established from the work of others and ourselves that the macroscopic and microscopic properties of water in fine capillaries can be greatly modified by the controlled addition of certain impurities. Indeed it has been demonstrated that (i) anomalous phase separation and density-temperature profiles can be obtained (refs. 12, 16, 18 and our own work) with solutions containing various salts, pump oils, silicon, hydrosols and other additives, such as Na and

B, which leave their characteristic residues after evaporation of the ordinary water; (ii) the published infrared spectra of polywater have a strong resemblance to those of a number of chemical solutions^{16-18,20-23} including carboxyl groups, bicarbonates, sulphates, sodium tetraborate, sodium and potassium nitrate, and particularly nitric acid and also human sweat. There can be little doubt that all the reported properties of anomalous water can be reproduced using an appropriate choice of additives. Clearly the exact impurity composition between different experiments and even between different samples must vary as do the reported properties of anomalous water. It is worth noting that different workers also use different concentrations; some use a very dilute mixture that exists before complete evaporation while others use only the residue.

On the question of organic impurities, Derjaguin seems content to deduce from the analysis in collaboration with V. L. Talrose that "one can state that the amount of organic compounds one can consider probable in the columns of water is much smaller than the amount necessary to explain the marked differences from normal water" (our italics). Apart from the vague wording of this statement, two important issues are involved. First, implicit in Derjaguin's thinking is that, if a certain concentration of impure water in bulk does not deviate from ordinary water, this will be true for the same concentration of impure water in fine capillaries (note that he uses this type of assumption with his "quartz powder experiment"). Knowing the considerable work (much of it by Derjaguin himself) on the restructuring capabilities of both surfaces and impurities (see refs. 24 and 25), ought one to expect the microscopic properties of impure solutions in fine capillaries to be reflected in the bulk state? Here Derjaguin seems to be in danger of falling into a pit of his own making. Second, the implication in the statement that the impurity concentration is small has certainly not been supported by analysis. According to Davis²⁰, the analysis of Talrose on Derjaguin's samples revealed enormous concentrations of organic substances (not unlike human sweat). Derjaguin, however, does not mention this.

Derjaguin's experiments cover many years of work using several methods of production. He talks of purity tests in connexion with a later purer apparatus, yet as far as we know all his published data stem from work with the earlier impure apparatus. This point is vital and must be clarified.

On more specific points too there seem to be several implicit contradictions. The dexterity used in the surface tension measurements was apparently sufficient to detect a 3% rise for water II concentrations of 20% by weight! Yet elsewhere he dismisses another experiment (the effect of perturbation on viscosity) simply as having "assigned too much weight to the result". In addition, we wonder how the viscosity results might be affected by the "spontaneous motion" effect—an effect that Derjaguin later describes as the sudden motion of an anomalous column when closer to one end of a capillary (that is, as it would be in a viscosity measurement). Also, how are these effects (and others like "daughter column production", distillation and so on) affected by the surface migration and vapour transport of both water and impurities? We know from the experiments of others^{16,17} as well as ourselves that these factors cannot be neglected. One could raise many more objections specifically aimed at individual experiments. However, for brevity here, we must regard them as of minor importance compared with issues already mentioned.

Derjaguin cites work in progress in the United States, Britain and Belgium to support his case. In fact his supporters are in a minority. Lippincott *et al.*, as we have shown, admit to the presence of impurities in addition to carbon, and their infrared spectra suspiciously resemble those of several solutions, particularly nitric acid. The work of Pethica *et al.*²⁶ which Derjaguin regards as partially confirmatory is no

longer regarded as such by at least one of the authors¹⁵. Our early (impure) work too could have been said to have confirmed Derjaguin's results.

Is it conceivable that Derjaguin and his collaborators alone have found the formula for producing pure anomalous water, while all others at best make impure imitations? Were this so, then, by virtue of the apparent ease of its production in 100 μm capillaries (production time of only 1 h or so), the ease of its distillation from small capillaries into a larger tube, and its great stability, this would make anomalous water an ideal substance for collective preparation. If this were so, a simple calculation shows that in the years of Derjaguin's experiments the production of a beaker full or even test-tube full of the substance should present no problem. Such a sample could then be made available for rigorous widespread examination.

Some of the work supporting the case for anomalous water may at first seem impressive. Closer examination, however, shows that much of the published work is careless, inconclusive and often misleading, if not wildly conjectural. Our final conclusions are: first, the observed properties of so-called anomalous water are simply the result of either gross impurities or impurity effects enhanced by surface proximity. Second, if sufficient precautions are taken to eliminate the appropriate contaminants, then the capillary products can become indistinguishable from ordinary water. Third, the different anomalous properties reported merely reflect the different types and levels of impurities characteristic of the methods of preparation. Fourth, although one can never dogmatically deny the existence of something not obtained, we think it highly unlikely that a pure anomalous water can exist.

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Purification of an Altered DNA Polymerase from an *E. coli* Strain with a *pol* Mutation

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DNA polymerase purified from an *E. coli* strain with a *pol* mutation has been shown to differ from purified wild type enzyme in several characteristics. This result demonstrates that the *pol* gene is the structural gene for DNA polymerase.

CELL-FREE extracts from the *Escherichia coli* mutant *polA1*, which has been isolated by De Lucia and Cairns¹, incorporate less than 1% as much deoxynucleoside triphosphate into acid-precipitable material as extracts of the parent strain in identical conditions. Although the mutation has been mapped and is an amber mutant², there is as yet no direct proof that the *polA1*

lesion is actually in the structural gene which codes for DNA polymerase. The mutation might therefore increase the level of some nuclease, thereby destroying the product of the polymerase (although this is unlikely because *polA1* is recessive^{1,2}); it might affect the extraction of the enzyme, although not its structure *in vivo*; or a promoter element might be mutated so as to produce fewer polymerase molecules of unaltered structure.

Moses and Richardson (personal communication) have been unable to detect any abnormality of the DNA polymerase purified from several suppressed derivatives of the amber *polA1* mutation. Cairns and De Lucia (personal communication) have isolated five additional *pol* mutants, and transduction with phage P1 has shown that these map at the same site on the chromosome as *polA1*³. Preliminary studies in this laboratory (Peacey, personal communication) have shown that extracts of one of these mutants, *pol-6*, prepared by sonic disruption, contain about 20% of wild type DNA polymerase activity, which is partially destroyed by preincubation at temperatures which have little effect on wild type extracts. The *pol-6* cells do not themselves seem to be temperature-sensitive in their

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growth or in the methylmethane sulphonate (MMS) sensitivity characteristic of all *pol* mutants³.

We have found that, unlike the wild type enzyme, the DNA polymerase partially purified from *pol-6* cells is less active when assayed at 52° C than at 37° C. We have therefore purified the enzyme from *pol-6* cells, and report now that the purified enzyme retains this property, as well as differing in other ways from wild type enzyme. This demonstrates that the *pol* mutation is in the structural gene for DNA polymerase.

Large Scale Purification

We have purified DNA polymerase according to published procedures^{4,5} using batches of log phase cells. The enzyme from wild type *E. coli* K-12 was first purified from 600 g of cells. The purified enzyme fraction (fraction VII) amounted to 4.4 mg of protein, which was essentially homogeneous when analysed in SDS-polyacrylamide gel electrophoresis (Fig. 1). This polymerase preparation was highly active in the assay system described by Richardson, Schildkraut, Aposhian and Kornberg⁵; its properties were exactly the same as those described by the previous investigators^{4,5} for the enzyme purified from *E. coli* B.

We have used the same protocol to purify polymerase from several batches of *pol-6* cells. In each case, the MMS sensitivity of the cells was tested at the time of collection. The polymerases from wild type and *pol-6* cells fractionated in an identical manner during the first five steps of the purification (Table 1), which resulted in a twenty to forty-fold increase in specific activity in each case. The absolute specific activities of the *pol-6* fractions were consistently lower than the corresponding wild type fractions (Table 1).

The behaviour of the two enzyme preparations differed significantly in step VI of the purification⁴, which consists of elution with a potassium phosphate gradient from a phosphocellulose column (Table 1). Fig. 2 shows phosphocellulose chromatograms of fraction V of wild type and mutant extracts. The wild type polymerase activity, indicated by histogram bars, elutes in the gradient fractions which contain a major protein peak. In contrast, the *pol-6* polymerase appears in fractions with a slightly greater phosphate concentration. The R_F s of elution with respect to the phosphate gradient were 0.34 for wild type and 0.36 for *pol-6* polymerase. This difference is small but reproducible. Although we did not find any significant losses of wild type polymerase, as much as two-thirds of the mutant polymerase activity was lost in the column front. This material had the same low specific activity and sensitivity to high temperatures as the material applied to the column and as the fractions of peak activity eluted from it. When it was rechromatographed it behaved identically. We do not at present understand the basis for this phenomenon.

Fraction VI was further purified by chromatography through a column of 'Sephadex G-150' (Table 1, fraction VII). Although the procedure of Jovin *et al.*⁴ utilizes a column of 'Sephadex G-100' for preparation of fraction VII from wild type cells, we chose the larger pore size to try to achieve a better separation of *pol-6* polymerase from the impurities found in the phosphocellulose fractions at $R_F=0.36$.

In the final purification steps the wild type and mutant preparations differ markedly in their response to poly dAT and calf thymus DNA as templates. Poly dAT is a much

Table 1 Purification of DNA Polymerase from Wild Type *E. coli* K-12 and from the *pol-6* Mutant

Fraction	Protein (mg/ml.)	Specific activity at 37° C*	Assay template	Ratio of activities at 51° C and 37° C
<i>A, Wild type</i>				
I Crude	20	3.7	Calf thymus DNA	1.6
II Streptomycin	16.8	14.7	Calf thymus DNA	1.5
III Autolysis	2.9	—	Calf thymus DNA	—
IV Ammonium sulphate	19.4	203	Calf thymus DNA	1.4
V DEAE cellulose	8.6	166	Calf thymus DNA	1.8
		360	poly dAT	1.6
VI Phosphocellulose	3.6	4,200	poly dAT	1.3
VII 'Sephadex G-100'	2.7	330	Calf thymus DNA	1.7
		3,000	poly dAT	2.4
<i>B, pol-6</i>				
I Crude	19.0	1.7	Calf thymus DNA	0.4
II Streptomycin	5.6	3.5	Calf thymus DNA	0.4
III Autolysis	3.0	6.7	Calf thymus DNA	0.4
IV Ammonium sulphate	14.0	33.3	Calf thymus DNA	0.4
V DEAE cellulose	5.0	52.8	Calf thymus DNA	0.33
		3.1	poly dAT	—
VI Phosphocellulose	4.35	30	Calf thymus DNA	0.24
		2.8	poly dAT	—
VII 'Sephadex G-150'	1.76	70	Calf thymus DNA	0.60
		14	poly dAT	0.33

The assays were modified from the proportions described by Richardson *et al.*⁵. Assays were conducted at 37° C or at 51° C for 30 min.

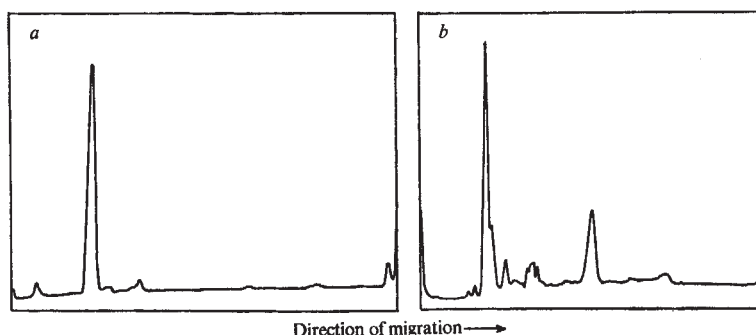
Assay with calf thymus DNA template: 6.54 nmol dGTP, dCTP and dTTP; 3.26 nmol ³H-dATP (2×10^{-2} Ci/mol); 1.63 μ mol MgCl₂; 0.33 μ mol mercaptoethanol; 5.76 μ mol glycine pH 9.2 and 0.26 nmol "activated" calf thymus DNA † plus enzyme ‡; total volume of 0.20 ml. Assay with poly dAT template: 9.5 nmol dTTP, 4.75 nmol ³H-dATP (1×10^{-2} Ci/mol), 2.36 μ mol MgCl₂, 0.475 μ mol mercaptoethanol, 20 μ mol potassium phosphate pH 7.4, 3.57 nmol dAT copolymer plus enzyme; total volume of 0.20 ml. Each assay was tested for linearity of ³H-dATP incorporation with time by removing 50 μ l. samples at zero time and every 10 min thereafter. These samples were spotted on Whatman 3MM filter paper disks which were washed thoroughly with cold 5% trichloroacetic acid (TCA) containing 1% sodium pyrophosphate. The TCA was extracted with ethanol and the disks were dried. Radioactivity retained on the disks was estimated by counting them in a liquid scintillation counter. This method is a modification of that of Bollum⁷. Deoxyribonucleoside triphosphates and calf thymus DNA were obtained from Calbiochem; poly dAT from Miles; ³H-dATP from Schwarz Bioresearch; other materials from standard laboratory supply houses.

* Specific activities are expressed as an arbitrary unit proportional to the net number of ³H atoms incorporated into TCA precipitable material in 30 min at 37° C per mg of enzyme protein.

† Activated calf thymus DNA was prepared by limited nuclease digestion of the DNA with pancreatic DNAase as described by Richardson⁸.

‡ The enzyme was diluted with the Tris-ammonium sulphate-mercaptoethanol-bovine serum albumin buffer described by Richardson *et al.*⁵.

Fig. 1 Densitometer tracings of SDS-polyacrylamide gels after electrophoresis of purified DNA polymerase preparations. SDS-polyacrylamide gels were prepared, run and stained according to the procedure of Weber and Osborn⁶. After destaining, the gels were scanned directly using a Joyce-Loebl ultraviolet scanning densitometer. Migration of the protein is from left to right of the figure. *a*, 14 μ g of wild type DNA polymerase fraction VII. Full scale A_{260} is 2.0. *b*, 18 μ g of *pol-6* DNA polymerase fraction VII. Full A_{260} is 1.0.



better template than calf thymus DNA for the wild type enzyme in fractions VI and VII⁵, whereas it is a very poor template for the mutant enzyme fractions (Table 1). This means that calf thymus DNA must be used to assay these fractions from *pol-6* cells, so that little purification seems to be achieved in the final steps. But the same is true of the wild type preparation if calf thymus DNA is employed in place of poly dAT.

The mutant enzyme remains sensitive to high temperature throughout its purification. The ratio of activity at 51° C to that at 37° C varied between 0.24 and 0.60 for the *pol-6* enzyme; and between 1.3 and 2.4 for the wild type fractions, as shown in Table 1.

Behaviour of the *pol-6* Polymerase

A comparison of the properties of enzyme fractions from *pol-6* and wild type cells shows that the *pol-6* enzyme represents a slightly modified form of DNA polymerase. The possibility that the low specific activity of the mutant preparation results from a highly effective nuclease rather than an inefficient polymerase can be tested by mixing experiments. First, crude extracts of the two types of cells were mixed and assayed together. Second, fraction VII of wild type polymerase was assayed in the presence of each of the seven fractions of the *pol-6* enzyme. In no case did the mutant enzyme fractions inhibit wild type activity. In addition, the higher specific activity

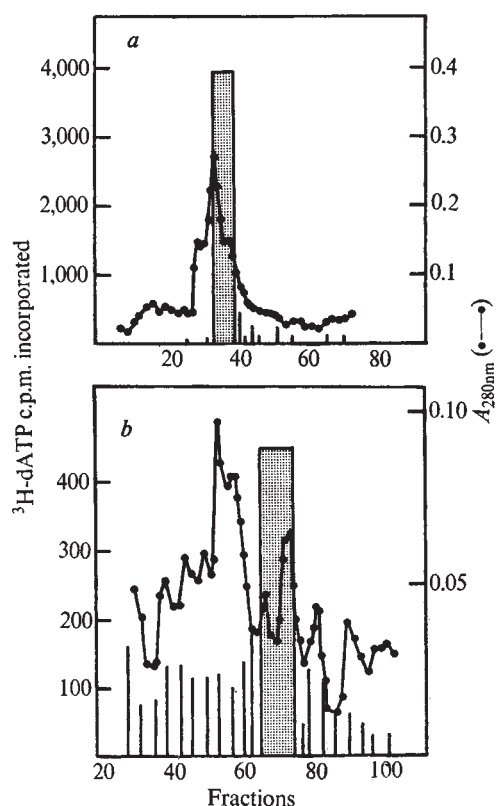


Fig. 2 Phosphocellulose chromatograms of fraction V of wild type (a) and *pol-6* (b) DNA polymerase preparations. Phosphocellulose chromatography of the DNA polymerase fraction V was carried out as described by Jovin *et al.*⁴. The protein was eluted from the phosphocellulose columns with a gradient of potassium phosphate pH 6.5. Fractions were collected and their absorbance at 280 nm was measured. Dilutions of these fractions were then tested for DNA polymerase activity using the assay system described in Table 1. The fractions containing DNA polymerase activity were pooled for further purification as fraction VI; their aggregate activities are indicated as a single histogram bar in each case. The histogram bars represent counts per minute of ³H-dATP incorporated at 30 min in the assay of that particular fraction. — — — represents A_{280} .

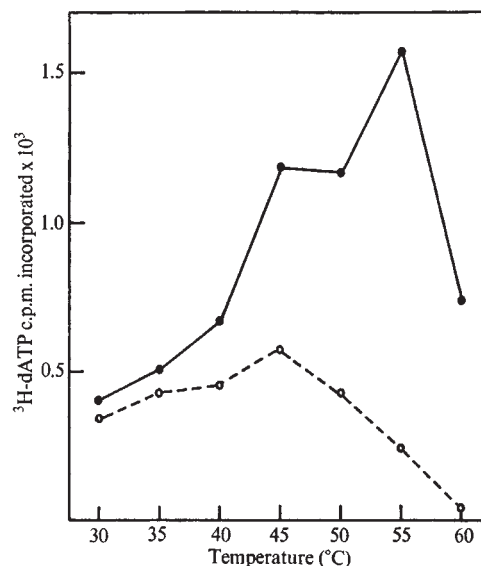


Fig. 3 Temperature profile of enzyme activity of wild type and *pol-6* DNA polymerase fraction VII. The purified DNA polymerase from wild type and *pol-6* was assayed with the calf thymus DNA primed assay described in Table 1 at temperatures between 30° C and 60° C. The final concentration of wild type polymerase in the assay was 0.1 µg/ml., that of the *pol-6* polymerase was 0.7 µg/ml. The incorporation of ³H-dATP into TCA precipitable material after 30 min is plotted against the temperature. ●—●, Wild type incorporation; ○—○, *pol-6* incorporation.

of the wild type enzyme at 51° C was maintained in the presence of the mutant enzyme. Table 2 summarizes the results of such a mixing experiment for the two final fractions.

The temperature optima of the purified enzymes were determined by assaying with calf thymus DNA as template over the range 30° C to 60° C. Fig. 3 summarizes our data. The wild type polymerase shows maximal activity at 55° C, whereas the optimum for the *pol-6* polymerase is 10° C lower.

The *pol-6* polymerase is immunologically similar to wild type DNA polymerase. Both enzymes react with antiserum against wild type *E. coli* B polymerase in complement fixation tests, and their enzyme activity is blocked by incubation of the reaction mixtures in the presence of antiserum. We measured the conformation and stability of the enzymes by a sensitive complement fixation procedure⁹, which showed that the melting temperature of the *pol-6* enzyme is 5° C lower than that of the wild type enzyme. These data indicate that there are basic similarities in the structure of the wild type and mutant enzymes but that they differ in conformational detail.

Table 2 Mixing Experiment using Fraction VII Polymerase from Wild Type *E. coli* and the *pol-6* Mutant

Enzyme	Final enzyme concentration (µg/ml.)	Template	³ H-dATP incorporation (c.p.m.)			Ratio 51° C / 37° C
			at 37° C	at 51° C		
(a) Wild type	0.102	Calf thymus DNA	373	1,026		2.8
		poly dAT	453	793		1.7
(b) <i>pol-6</i>	0.730	Calf thymus DNA	511	413		0.8
		poly dAT	75	9		0.12
(c) Mixture of wild type + <i>pol-6</i> polymerases	0.102	Calf thymus DNA	1,107	1,538		1.4
		poly dAT	444	806		1.8
(d) Sum of (a)+(b)	0.730	Calf thymus DNA	884	1,439		1.6
		poly dAT	548	802		1.4

The purified polymerases from the wild type and the *pol-6* mutant were assayed separately and as a mixture at both 37° C and 51° C. Final concentrations of the two enzymes in the mixture, (c), were the same as in the assays of the individual enzymes, (a) and (b). The incorporations in the individual assays were summed, (d), for comparison with the results of the assay of the mixture.

Kornberg and Geft¹⁰ have recently isolated a DNA polymerizing enzyme in soluble form from *polA1* cells. It is most unlikely that this is the enzyme we have purified from *pol-6* cells. Their enzyme depends on the presence of ATP for activity in crude extracts, and catalytic activity is only abolished by concentrations of anti-DNA polymerase serum in a hundred-fold excess of that needed to block the wild type enzyme. By contrast, the activity in *pol-6* extracts can be assayed in the absence of ATP, and the purified product reacts strongly with anti-DNA polymerase.

We believe that our results demonstrate that the *pol-6* mutation is in the structural gene for DNA polymerase. The exact nature of the structural change that has resulted from the mutation is not clear at present. Our present data imply that only slight conformational changes are involved, but confirmation of this requires further experiments. Gross and Peacey (personal communication) have recently isolated an F' factor carrying the amber *polA1* mutation and have shown that the F' factor does not complement the polymerase defect in *pol-6* cells. It is therefore probable that *polA1* is also a mutation of the structural gene for DNA polymerase.

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Novel Species of tRNA

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A species of tRNA^{Gly} from *Staphylococcus epidermidis*, which participates in peptidoglycan synthesis but not in protein synthesis, has been purified to homogeneity. It contains only a single modified base, 4-thiouridine.

In certain species of bacteria, aminoacyl-tRNA apparently serves multiple synthetic roles. In addition to its familiar function in protein synthesis, it provides the amino-acid residues forming the interpeptide bridge of some bacterial cell walls¹⁻⁴. Still other unusual functions of aminoacyl-tRNAs are their roles in aminoacyl phosphatidylglycerol synthesis^{5,6} and in the terminal addition of amino-acid residues to proteins^{7,8}.

In the peptidoglycan of the cell wall of *Staphylococcus aureus*, the interpeptide bridge is pentaglycine, while in *S. epidermidis* both glycine and serine residues are found. For both glycyl-tRNAs from *S. aureus*⁹ and the seryl-tRNAs from *S. epidermidis*², multiple isoaccepting species can be separated and all species function in peptidoglycan synthesis; but in each case a species of tRNA fails to participate in protein synthesis. We now report a study of the isoaccepting species of tRNA^{Gly} from *S. epidermidis* and find a parallel situation. Again there is one species (tRNA^{Gly}) which functions only in peptidoglycan synthesis.

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Isoaccepting Species of tRNA^{Gly}

Crude tRNA from *S. epidermidis* was resolved into three fractions by chromatography on DEAE-cellulose (Fig. 1). The first fraction (tRNA^{Gly}_I) eluted ahead of the main absorbance peak and after rechromatography on hydroxylapatite (Fig. 2), gave a major peak of ultraviolet absorbing material coincident with glycine acceptance. This material had a specific activity of 1,300 pmol of glycine (accepted per absorbance unit).

The second fraction (tRNA^{Gly}_{II}) was further resolved into two peaks (tRNA^{Gly}_{IIA} and tRNA^{Gly}_{IIB}) by chromatography on DEAE-cellulose at pH 5.0 (Fig. 3), while the third fraction (tRNA^{Gly}_{III}) has not been further resolved during extensive purification.

All four isoaccepting species participated in the incorporation of ¹⁴C-glycine into the interpeptide bridges of the peptidoglycan (Table 1).

Two messengers have been used to test participation in protein synthesis *in vitro*. Phage f2 RNA has been used such that the phage coat protein is the predominant product, and also phage T4 RNA, which codes for various proteins. With both messengers, glycyl-tRNA^{Gly}_{IIA}, glycyl-tRNA^{Gly}_{IIB} and glycyl-tRNA^{Gly}_{III} function in the incorporation of glycine into protein. But in the same conditions, we could not demonstrate the incorporation of glycine from glycyl-tRNA^{Gly}_I into protein using either messenger (Table 2). Glycyl-tRNA^{Gly}_I did not inhibit the *in vitro* assay because its addition to each of the other tRNA^{Gly} species failed to suppress incorporation from those species.

The trinucleotide-dependent binding of ¹⁴C-glycyl-tRNA^{Gly} to *Escherichia coli* ribosomes was not stimulated by the addition of the known glycine codons GGU, GGC, GGA, GGG. In identical conditions the binding of the other species of tRNA^{Gly}

Table 1 Utilization of ^{14}C -Glycyl-tRNA^{Gly} Species for Peptidoglycan Synthesis

Conditions	^{14}C -Glycyl-tRNA ^{Gly} (Total c.p.m. added)	Radioactivity incorporated into peptidoglycan (% incorporation)
Complete	tRNA ^{Gly} (9,500)	713 (7.5%)
Minus UDP-GlcNAc		66
Complete	tRNA ^{Gly} _{IIA} (9,000)	460 (5.1%)
Minus UDP-GlcNAc		70
Complete	tRNA ^{Gly} _{IIB} (8,000)	527 (6.6%)
Minus UDP-GlcNAc		85
Complete	tRNA ^{Gly} _{III} (10,000)	540 (5.4%)
Minus UDP-GlcNAc		100

Assay of peptidoglycan synthesis was carried out using the reaction mixtures described by Matsubashi, Dietrich and Strominger¹.

to *E. coli* ribosomes was stimulated by one or more of these trinucleotides (Table 3).

Analysis of tRNA^{Gly}

Table 4 shows our analysis of the nucleosides obtained from tRNA^{Gly} after enzymatic hydrolysis. It contained approximately equimolar amounts of uridine, guanosine, cytidine and adenosine. A single modified nucleoside, 4-thiouridine, was present in an amount equal to 0.65 residue per mol. No other minor nucleoside was detected in an amount greater than 0.1 residue per mol. Because dihydrouridine has no absorption at 260 nm, it could not be detected by this technique; its absence was established from the analysis of ^{32}P -labelled tRNA^{Gly} (ref. 14). Although the total tRNA from a *Mycoplasma* species has been shown to be deficient in minor bases¹⁵, this is the first isolation of a pure species of tRNA which is missing so many modified bases. Estimating terminal adenosine after alkaline hydrolysis¹⁴ gave a value of 1,300 pmol per mol of tRNA, in agreement with the value obtained for glycine acceptance. This value suggests that the chain length is about 85 residues.

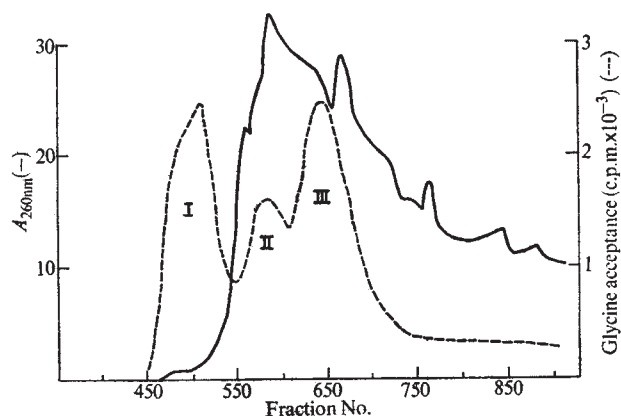


Fig. 1 Chromatography of crude tRNA from *Staphylococcus epidermidis* on DEAE-cellulose. Crude tRNA² (375,000 A_{260} units) in buffer (0.02 M Tris-HCl, pH 7.5, 0.001 M MgCl_2 , 0.005 M $\text{Na}_2\text{S}_2\text{O}_3$, 0.001 M EDTA) was applied to a column of DEAE-cellulose (Whatman DE-52), 100×6 cm, equilibrated with the same buffer. Elution was carried out with 16 l. of a linear gradient from 0.25 M to 0.45 M NaCl in the same buffer. Fractions of 20 ml. were collected and assayed for glycine acceptance using 0.025 ml. of a column fraction, 7 μmol of HEPES (pH 8.0), 0.55 μmol of MgCl_2 , 0.11 μmol of ATP, 0.2 μmol of 2-mercaptoethanol, 0.0085 μmol of mixed amino-acids minus glycine, 0.0016 μmol of ^{14}C -glycine (116 mCi/mmol, New England Nuclear Corp.) and 0.005 ml. of a crude enzyme preparation in a total volume of 0.08 ml. After incubation at 25°C for 30 min, trichloroacetic acid insoluble radioactivity was determined by standard procedures.

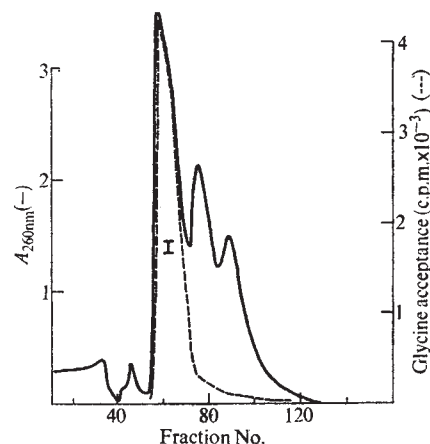


Fig. 2 Rechromatography of crude tRNA^{Gly} on hydroxylapatite. Crude tRNA^{Gly} (2,200 A_{260} units), ϵ eluted from the DEAE-cellulose column (Fig. 1), was applied to a column (100×2.5 cm) of hydroxylapatite¹⁰, equilibrated with 0.05 M sodium phosphate, pH 6.88. Elution was carried out with 3 l. of a linear gradient from 0.08 M to 0.15 M sodium phosphate, pH 6.88. Aliquots (0.1 ml.) of column fractions (20 ml.) were precipitated by the addition of H_2O (0.1 ml.) and ethanol containing 20% potassium acetate, pH 5.0 (0.5 ml.). The precipitate was collected by centrifugation, dissolved in H_2O (0.025 ml.) and assayed as described in the legend to Fig. 1.

The ultraviolet absorption spectrum of tRNA^{Gly} contained the intense absorption at 335 nm which is characteristic of tRNAs containing 4-thiouridine; but this absorption formed part of the leading edge of the main peak at 260 nm and no clear peak could be resolved in a variety of solvents. The absence of a distinct peak at 335 nm is unusual, and it may be that partially oxidized species of tRNA^{Gly} are contaminating the sample. We confirmed that the absorption at 335 nm was due to a thio-nucleotide by treating tRNA^{Gly} with cyanogen

Table 2 Utilization of ^{14}C -Glycyl-tRNA^{Gly} Species for Protein Synthesis

Messenger	^{14}C -Glycyl-tRNA ^{Gly} (total c.p.m. added)	Radioactivity incor- porated into protein (%)
f2	tRNA ^{Gly} (5,000)	197 (2)*
T4		114 (2)
f2	tRNA ^{Gly} _{IIA} (4,500)	1,580 (35)
T4		1,155 (26)
f2	tRNA ^{Gly} _{IIB} (6,000)	2,701 (45)
T4		1,472 (25)
f2	tRNA ^{Gly} _{III} (5,400)	2,620 (49)
T4		1,481 (27)

Assay of incorporation into protein was carried out as described by Lodish¹¹ except using a modified ribosome preparation¹².

* This level of incorporation is of the same order as the blank which has already been subtracted.

Table 3 Trinucleotide Dependent Binding of ^{14}C -Glycyl-tRNA^{Gly} Species to *E. coli* Ribosomes

^{14}C -Glycyl-tRNA ^{Gly}	No triplet	Radioactivity bound (c.p.m.)			
		GGG	GGA	GGU	GGC
tRNA ^{Gly}	45	42	44	49	40
tRNA ^{Gly} _{IIA}	76	312	234	172	96
tRNA ^{Gly} _{IIB}	74	270	238	174	76
tRNA ^{Gly} _{III}	283	248	251	677	726

Reaction conditions were as described by Nirenberg and Leder¹³, except that 0.04 M MgCl_2 was used. We thank Dr John Carbon for the samples of the trinucleotides.

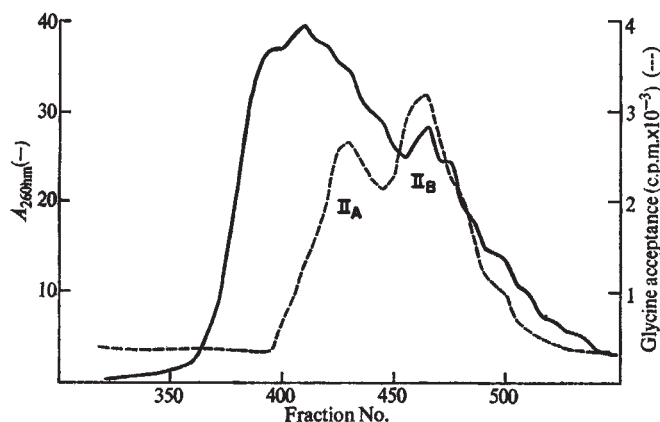


Fig. 3 Rechromatography of crude tRNA^{Gly} on DEAE-cellulose. Fractions enriched in tRNA^{Gly} (Fig. 1) (160,000 A₂₆₀ units) were made 0.05 M in sodium acetate, pH 5.0, and applied to a column (100 × 6 cm) of DEAE-cellulose, equilibrated with buffer (0.02 M sodium acetate, pH 5.0, 0.01 M MgCl₂, 0.005 M Na₂S₂O₃, 0.001 M EDTA). Elution was carried out with 16 l. of a linear gradient from 0.35 M to 0.55 M NaCl in the same buffer. Aliquots (0.025 ml.) of column fractions (20 ml.) were assayed as described in the legend to Fig. 1.

bromide^{17,18} which resulted in complete loss of absorption at 335 nm. This reaction specifically removes sulphur from thio-nucleotides^{17,18}.

Table 4 Base Analysis of tRNA^{Gly}

	Uridine	Guanosine (nmol per A ₂₆₀ of tRNA ^{Gly})	Adenine	Cytidine	4-Thiouridine
Analysis 1	28.9	27.0	27.2	27.8	0.9
Analysis 2	28.5	25.6	27.1	28.2	0.8
Average	28.7	26.3	27.1	28.0	0.85
Composition (residues per mol)	22.1	20.2	20.9	21.5	0.65 (1)

The analytical conditions were as described by Uziel, Koh and Cohn¹⁴. The composition was calculated from the glycine acceptance value of 1,300 pmol per A₂₆₀ unit.

The absence of minor bases, with the exception of 4-thiouridine, in tRNA^{Gly} raises several interesting questions. Its failure to participate in protein synthesis or to bind to ribosomes may reflect the absence of an essential minor base. For example, the common sequence G T ψ C must be missing, and although a tRNA missing ribothymidylic acid is known to function in protein synthesis¹⁹, there is some evidence that this sequence is essential for proper binding to ribosomes²⁰. But the tRNA is recognized by the glycyl-tRNA synthetase. A single glycyl-tRNA synthetase has been found in *S. aureus*²¹, and a single synthetase is also present in *S. epidermidis*. Clearly, minor bases (with the possible exception of 4-thiouridine) are unimportant in this respect. This observation agrees with the suggestion that the stem region of certain tRNAs is involved in recognition by the aminoacyl synthetase²². This region is devoid of minor bases in the tRNAs which have been sequenced so far.

Function of tRNA^{Gly}

The presence in both *S. aureus* and *S. epidermidis* of tRNAs which do not function in protein synthesis makes it unlikely that these are artefacts. Furthermore, they occur in normal quantities and reproducible amounts—in both organisms tRNA^{Gly} represents about 40% of the total tRNA^{Gly}. This species may be a precursor of one or more of the other tRNA^{Gly} species in which the sole modification has been the conversion of a uridine residue to 4-thiouridine. To account for the absence of further modification, it is then necessary to postulate

that this tRNA is sequestered in some manner for use in peptidoglycan synthesis. Alternatively, the 4-thiouridine residue may be located abnormally and might serve to protect the molecule from the other modifying enzymes. Finally, it is possible that tRNA^{Gly} represents a unique gene product with a discrete function in peptidoglycan synthesis. In this case, it must lack the necessary recognition sequences for the usual modifying enzymes (such as methylases). Its function in peptidoglycan synthesis would then result from its availability over other species of tRNA^{Gly}, which would be engaged in protein synthesis.

The bridge amino-acids, glycine and serine in *S. epidermidis*, occur in the peptidoglycan in an amount which is at least equal to the total amount of these amino-acids present in all of the cell protein²³. This occurs because the cell wall represents approximately 25% of the dry weight of the organism. It therefore seems reasonable that the bacterial cell has evolved a mechanism which ensures an adequate supply of these amino-acids for cell wall synthesis, and specifically in a form in which they cannot be used for protein synthesis.

That all four isoaccepting species of tRNA^{Gly} work equally well in the *in vitro* assay for peptidoglycan synthesis seems to indicate a certain lack of specificity. But the possible origin of tRNA^{Gly} suggests an explanation for this phenomenon. Originally, only tRNA^{Gly} species functional in protein synthesis would be present in the cell and competition would be necessary to utilize glycyl-tRNA^{Gly} for peptidoglycan synthesis. The "modification" of an existing species of tRNA^{Gly}, such that it no longer functioned in protein synthesis but still functioned in peptidoglycan synthesis, might then yield a selective advantage. No further advantage would be gained by losing the ability to utilize the other species of tRNA^{Gly} for peptidoglycan synthesis.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Collapsed or Neutron Star Companions of Bright Stars

Cameron¹ and Stothers² have suggested that the companion to ϵ Aurigae is a collapsed star. Epsilon Aurigae is one of several binary stars with large mass functions and invisible secondaries and so far the primaries of two of the brightest of these, ν Sgr (ref. 3) and β Lyrae, have been found to have helium-rich atmospheres. This shows that mass exchange has occurred, and that the visible star has been stripped down to its hydrogen deficient core by transferring matter into the vicinity of the former secondary. This explanation for ϵ Aur is the third of four possibilities discussed by Stothers. The proof of it would be the discovery of anomalously strong He I lines, such as $\lambda 5876$, which appear in cool helium rich stars like R Cr B (ref. 4). This spectroscopic check should be made.

Huang suggested an explanation for the low luminosity of the apparently more massive component of these systems when he showed that the geometrical eclipse of β Lyrae was consistent with the faint massive star being disk shaped (though with some central condensation⁵). I pointed out⁶ that the reduced central pressure of a disk compared with a sphere would result in a lower luminosity. On a time scale imposed by transport of angular momentum inside the disk and to the circumstellar shell, the massive star would become more circular and luminous, eventually evolving into a system like V356 Sgr in which both components are visible. The problems of the secular stability of disk shaped stars are not well understood, but there is no good theoretical basis for denying their existence.

In a system which has undergone mass exchange, the star that is expected to collapse first through fuel exhaustion is the lower mass star, the primary. It is more luminous, less massive, and only has helium to burn, whereas the other star is an almost zero age, normal composition star. The relative amounts of fuel available for the two stars, and the anomalous luminosities of the two components, ensure that the star which is visible for the time being will burn out in a small proportion of the main sequence lifetime of the disk star which is becoming visible.

If there are two or three stars brighter than fourth magnitude in this anomalous state, there should be about 100 bright stars in the more advanced state in which the primary has burned out; the new primary should be about the same luminosity as the old. The visible component will usually be an O or B star near the main sequence. This number of bright O or B stars is remarkably similar to the total number known. It is certainly consistent with half the known B star binaries being relatively young, and the other half having their secondaries fully evolved. Because a high proportion of B stars are known to be binaries, it is possible, but somewhat less likely, that these evolved systems are B stars which now appear to be single.

There are arguments that the present primaries of these binary systems will be very inefficient at shedding more mass. Mechanisms known to be effective seem to depend on the

presence of an outer hydrogen zone, either to provide a jump in molecular weight that extends the outer layers of the star, or to provide a shell source of energy. It seems that, with the hydrogen gone, the fate of these components will be implosion either to give a neutron star or a collapsed object. It would be useful if a selected group of the brightest B stars could be examined for pulsars at the same coordinates in order to examine these possibilities. A study of some of the nearest B stars by lunar occultation to discover whether the secondary component of single line binaries is indeed a visible star would also be of great interest. Close binary stars provide remarkably detailed information on the history and nature of their components. Full advantage of this fact should be taken in exploring the newer more exotic areas of astrophysics.

The evidence from infrared observations discussed by Cameron and Stothers might in principle be used to disentangle the radiation of the secondary from that of circumstellar gas and dust. Attempts to do this have so far been hampered by lack of understanding of the range of phenomena in single stars. Confusion has been added by the systematic errors of some of the earlier observations and the problems of disentangling these from genuine time variations of objects⁷. Observations of some of the binaries are reported in a tabulation of photometric observations⁸. The infrared studies are continuing but the other approaches suggested above may increase the rate of progress in the search for the physical properties of neutron stars and collapsed stars.

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Observed Abundance Distribution of Chemical Elements as a Test of Alfvén's Theory of the Origin of the Solar System

MAGNETIC fields act on the orbits of ions and Alfvén¹ suggested many years ago that this effect was important during the early history of the solar system. Neutral atoms and ions will follow different paths which may cause a separation effect. In the work described here, the ionization energy has been used as a parameter and the abundances of the chemical elements in ordinary chondrites, terrestrial magma and lunar rocks have been compared with solar abundances.

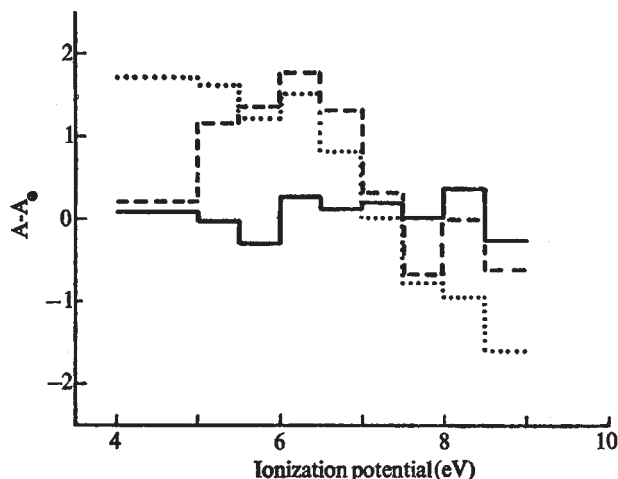


Fig. 1 The chemical abundances of the elements in ordinary chondrites (—), terrestrial magma (· · ·) and lunar rocks (---) relative to the solar values versus the ionization potential of the element. The ordinate is a logarithmic scale.

As the Sun contains nearly all the mass in the solar system there is good reason to believe that the solar abundances reflect the primordial chemical composition of the solar system. But this is not true for a few of the elements which are involved in solar energy production and for elements such as Li, Be and B which are destroyed at existing temperatures near the bottom of the solar convection zone. Such elements have been excluded from the comparison.

My work has been based on abundance data from several sources. Wedepohl² has compiled the abundances in ordinary chondrites and in terrestrial magmatic rocks. The latter is an average of different types of rocks, taking into account the terrestrial distribution of each type, thus representing the average chemical composition of the upper terrestrial magma. I have used the average value of the abundance data published by Morrison *et al.*³ for seven lunar rocks from the Apollo 11 mission. Except for a few elements, the chemical compositions of the different rock samples are nearly the same. Hauge and Engvold⁴ have compiled solar abundance data. For some elements, there are still discrepancies in the solar abundances as derived by different methods, but for the comparison reported here the uncertainty in the solar abundances is of minor importance.

In this communication the abundance of elements in ordinary chondrites, terrestrial magma and lunar rocks is given relative to the solar value for each element. The solar abundances are adjusted in value such that on average they come close to the abundances in the other objects.

Ordinary chondrites, terrestrial magma and lunar rocks have been investigated as follows: the relative abundances for all elements within a certain ionization potential interval and the average values of their logarithms⁴ have been derived. The number of elements within each interval varies from two to nine. All elements were given the same reliability weight. A histogram of the relative abundances versus ionization potential is shown in Fig. 1.

The available abundance data do not necessarily represent the mean abundance of the object. The terrestrial core, for example, has a high iron content; the iron abundance I have used is 3.54% of the total mass. If, however, this value is increased by a factor of ten, the histogram level in Fig. 1, between 7.5 and 8 eV, should be 0.125 dex higher. Such a change does not alter the general trend of the diagram. When each element is plotted in the diagram, there is considerable scatter of the points, but about 90% of the elements are within the relative abundance range of 0.1 to 10 of the average value. By using many trace elements, representative mean values should be obtained.

The histograms based on forty-four elements show clear trends. In the case of terrestrial magma and lunar rocks, the step curve falls off toward higher ionization potentials corresponding to relative abundance depletions of 10^{-2} to 10^{-3} . For the elements with low ionization potentials the terrestrial abundance exceeds that of lunar rocks by a factor of 30. These results support Alfvén's¹ ideas about the formation of the Moon and the Earth.

The chondrite abundances seem to be independent of the ionization potential, and chondrites may reflect the composition of the original matter. As mentioned previously, with some exceptions, we should expect to find this composition on the Sun. The chondrite histogram in Fig. 1 shows a depression at 5–6 eV. This depletion can easily be explained by erroneous determination of the solar abundances of In and rare earth elements. At higher ionization potentials, the solar abundances of Cd, W, Os and Ir may be doubtful. I suspect that the chemical abundances in chondrites are even closer to the "true" solar values, and hence to the primordial matter, than Fig. 1 suggests. Because the chemical composition of chondrites is known, we can compare the lunar and terrestrial data with the chondrite values (Fig. 2). The histogram curves for the relative lunar and terrestrial abundances now become smoother, and there is a systematic trend between lunar and terrestrial data which may be significant. Above 5.5 eV the terrestrial abundances are depleted compared with the lunar abundances, while for lower ionization potentials the opposite situation obtains.

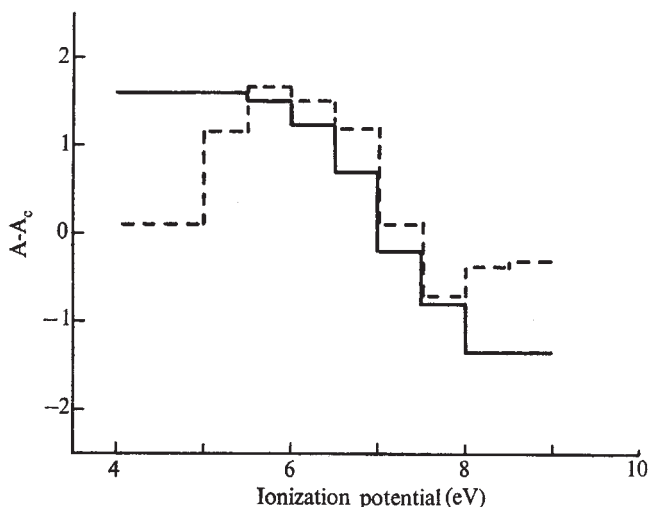


Fig. 2 The chemical composition in terrestrial magma (—) and lunar rocks (---) as a function of the ionization potential. Abundances are given relative to that of ordinary chondrites.

The investigations reported here lead to the following tentative conclusions. Ionized matter in magnetic fields played an important part in the formation of the Earth and the Moon, which were formed in different ionization conditions.

I shall assume that the energy which caused the ionization increases towards the Sun. Then the Moon must have been formed on the inner side of the Earth. This assumption is also supported by the high density of Mercury which may be due to a higher iron content, while the less dense planet Mars should be overabundant on elements with low ionization potentials such as Na, Al, K and Ca.

Considering the ionization potentials only, I cannot give a complete answer to these questions. Many other physical and chemical properties of matter must be taken into account in a theory for the solar system.

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Pyrolytic Graphite Microcalorimeter for the Measurement of X-Ray Absorbed Dose

To meet the need for absolute standards of absorbed dose for X-rays, the National Physical Laboratory together with other national laboratories is investigating methods based on microcalorimetry. The ideal would be to measure "local" absorbed dose in materials such as soft tissue to which water would represent a fairly satisfactory approximation. However, as Report 14 of the International Commission on Radiation Units and Measurements¹ states, "the direct determination of the absorbed dose at a point in a liquid (for example, water) has not been feasible primarily because of difficulties of achieving homogeneity of the irradiated system". An indirect method, in which the absorbed dose is determined in a solid "reference" material, is thus at present used.

The choice of a reference material is severely restricted by the desirability of avoiding not only endothermic and exothermic reactions but also significant energy storage (as, for example, by lattice deformation) and materials of high atomic number (which give rise to difficulties in deriving the absorbed dose in a material of low effective atomic number such as water). Thus¹ polystyrene and carbon (as graphite) "are presently considered to be the most suitable materials for absorbed dose calorimetry". Of these materials, carbon would be the obvious choice as a pure elemental substance were it not for the disadvantage that its electrical resistivity ($\sim 10^{-3} \Omega\text{cm}$ as ordinary graphite) is too small for it to be used for calibration purposes by means of Joule self-heating, and because of reservations¹ about possible reactions induced by radiation associated with persistent gas adsorption. We believe that both these objections may be overcome by the use of pyrolytic graphite. Gas adsorption in this form of graphite, which is virtually impermeable, is negligible in comparison with ordinary graphite, while the marked anisotropic electrical properties can be exploited to achieve calibration self-heating.

Microcalorimeters of the "local" absorbed dose type are usually calibrated by comparing the temperature rise resulting from irradiation in a thermally isolated portion of the chosen material (the "absorber") with the temperature rise produced by a known amount of electrical energy dissipated in a small heater winding either embedded in or attached to the absorber. This procedure needs careful scrutiny for the possible effects of temperature gradients and accompanying thermal transients present in the case of electrical heating (owing to the discrete nature of the heating source), but largely absent during the "through-the-bulk" heating by X-ray irradiation. Laughlin and Genna² have stressed this need to consider carefully such

effects, Petree and Lamperti³ have discussed methods of correcting for them and Domen⁴ has described a method of minimizing them by means of what he terms a "heat loss compensated" calorimeter. It has previously been assumed that inhomogeneous calibration heating is unavoidable in the case of a material such as ordinary graphite since (unlike plastic materials loaded with a small amount of carbon to make them partially conducting) its resistivity is so low that it is "not suitable for use as [its own]—heating resistor" (ref. 1). Nevertheless, "through-the-bulk" heating during calibration is clearly very desirable.

Table 1 Physical Properties of Pyrolytic Graphite

Density	2.2 g/cm ³
Purity	Metallic impurities typically 10 p.p.m.
Specific heat	$\sim 0.7 \text{ J/g deg. C}$
<i>a</i> direction thermal conductivity	$\sim 3.8 \text{ J cm/cm}^2\text{s deg. C}$
<i>c</i> direction thermal conductivity	$\sim 0.025 \text{ J cm/cm}^2\text{s deg. C}$
<i>a</i> direction electrical resistivity	$\sim 400 \times 10^{-6} \Omega\text{cm}$
<i>c</i> direction electrical resistivity	$\sim 0.7 \Omega\text{cm}$
<i>a</i> direction emissivity	0.8 to 0.9
<i>c</i> direction emissivity	0.5

To permit carbon to be used as the calorimetric material as its own electrical resistor, we are investigating the use of pyrolytic graphite. This material is a pure polycrystalline form of graphite exhibiting a high degree of preferred orientation, and in consequence it has marked electrical and thermal anisotropy, the electrical and thermal conductivities in the *c* direction being orders of magnitude less than the corresponding quantities in an *a* direction (the latter being any direction at right angles to the *c* direction). Details of the method of production and structure of pyrolytic graphite have been well summarized⁵⁻⁸. Table 1 sets out typical values of the physical properties of "as-deposited" pyrolytic graphite of special interest in the present context. We are investigating these properties, more especially the degree of uniformity both of density and of resistivity in the *c* direction, the latter being the quantity which it is hoped to exploit for electrical self-heating. We are also investigating the problems associated with machining the material with sufficient precision without significantly disturbing resistivity in the *c* direction. An "absorber" of pyrolytic graphite of the dimensions commonly used in a microcalorimeter can be arranged to have a resistance of several ohms which, although not low enough to create serious difficulties, is sufficiently low to raise questions about the fabrication of suitable electrode surfaces and also the mode of attachment of electrical connexions, to which we are also giving attention. Further, we have concerned ourselves with the possible effects of prolonged exposure to high energy photons on, for example, resistivity in the *c* direction. We are also investigating possible anisotropy of interaction with X-rays.

A comprehensive theoretical study of the application of pyrolytic graphite in a "local" absorbed dose microcalorimeter has been completed which has led to a prototype design, now under construction. The exploitation of the *c* direction resistivity to provide Joule self-heating for calibration purposes promises a considerable simplification in the design and use of a multi-jacketed adiabatic or quasi-adiabatic system; for example, the usual servo-heating system for the "jacket" can be eliminated. Again, the thermal anisotropy of the material can be exploited in the practical layout of the calorimeter, while the fact that pyrolytic graphite is virtually impermeable will enable it to be used as the vacuum jacket if so desired. We plan to operate the calorimeter on a basis in which irradiation and calibration heating go on simultaneously.

The proving of pyrolytic graphite as a suitable calorimetric reference material will contribute to the establishment of an absorbed dose standard for the United Kingdom, while the eventual intercomparison of such a standard with other stand-

ards should contribute to our knowledge of the nature and magnitude of the systematic errors in this field.

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Convection Plumes in the Lower Mantle

THE concept of crustal plate motion over mantle hotspots has been advanced¹ to explain the origin of the Hawaiian and other island chains and the origin of the Walvis, Iceland-Faroe and other aseismic ridges. More recently the pattern of the aseismic ridges has been used in formulating continental reconstructions². I have shown³ that the Hawaiian-Emperor, Tuamotu-Line and Austral-Gilbert-Marshall island chains can be generated by the motion of a rigid Pacific plate rotating over three fixed hotspots. The motion deduced for the Pacific plate agrees with the palaeomagnetic studies of seamounts⁴. It has also been found that the relative plate motions deduced from fault strikes and spreading rates agree with the concept of rigid plates moving over fixed hotspots. Fig. 1 shows the absolute motion of the plates over the mantle, a synthesis which satisfies the relative motion data and quite accurately predicts the trends of the island chains and aseismic ridges away from hotspots.

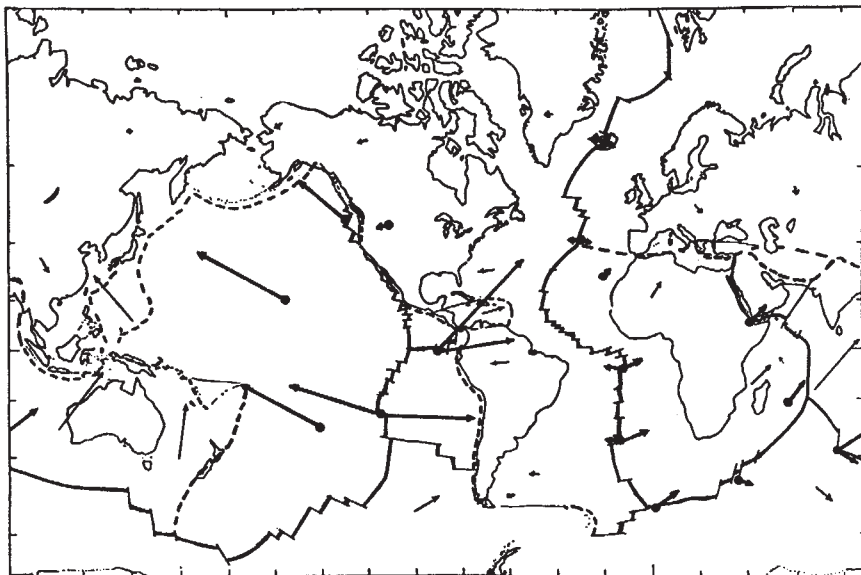
I now propose that these hotspots are manifestations of convection in the lower mantle which provides the motive force for continental drift. In my model there are about twenty

deep mantle plumes bringing heat and relatively primordial material up to the asthenosphere and horizontal currents in the asthenosphere flow radially away from each of these plumes. The points of upwelling will have unique petrological and kinematic properties but I assume that there are no corresponding unique points of downwelling, the return flow being uniformly distributed throughout the mantle. Elsasser has argued privately that highly unstable fluids would yield a thunderhead pattern of flow rather than the roll or convection cell pattern calculated from linear viscous equations. The currents in the asthenosphere spreading radially away from each upwelling will produce stresses on the bottoms of the lithospheric plates which, together with the stresses generated by the plate to plate interactions at rises, faults and trenches, will determine the direction in which each plate moves.

Evidently the interactions between plates are important in determining the net force on a plate, for the existing rises, faults and trenches have a self-perpetuating tendency. The plates are apparently quite tough and resistant to major changes, because rise crests do not commonly die out and jump to new locations and points of deep upwelling do not always coincide with ridge crests. (For example, the Galapagos and Réunion upwellings are near triple junctions in the Pacific and Indian Oceans. Asthenosphere motion radially away from these hotspots would help to drive the plates from the triple junctions, but there is considerable displacement between the "pipes to the deep mantle" and the lines of weakness in the lithosphere which enable the plates to move apart.) Also, a large isolated hotspot such as Hawaii can exist without splitting a plate in two. I believe it is possible to construct a simple dynamic model of plate motion by making assumptions about the magnitude of the flow away from each hotspot and assumptions about the stress/strain rate relations at rises, faults and trenches. Such a model has many possibilities to account for past plate motions; hotspots may come and go and plate migration may radically change the plate to plate interactions. But the hotspots would leave visible markers of their past activity on the seafloor and on continents.

This model is compatible with the observation that there is a difference between oceanic island and oceanic ridge basalts^{5,6}. It suggests a definite chain of events to form the island type basalt found on Hawaii and parts of Iceland. Relatively primordial material from deep in the mantle rises adiabatically up to asthenosphere depths. This partially fractionates into a liquid and solid residual, the liquid rising through vents to form the tholeiitic part of the island. The latter alkaline "cap rocks" would be generated in the lithosphere vent after plate motion had displaced the vent from the "pipe to the deep

Fig. 1 The arrows show the direction and speed of the plates over the mantle; the heavier arrows show the plate motion at hotspots. This synthesis was based on relative plate motion data (fault strikes and spreading rates) and predicts the directions of the aseismic ridges/island chains emanating from the hotspots.



mantle". In contrast, the ridge basalts would come entirely from the asthenosphere, passively rising to fill the void created as plates are pulled apart by the stresses acting on them. The differences in potassium and in rare earth pattern for island type and ridge type basalts may be explained by this model. Moreover, the 2 billion year "holding age" advocated by Gast⁷ to explain lead isotope data of Gough, Tristan da Cunha, St Helena and Ascension Islands may reflect how long the material was stored in the lower mantle without change prior to the hotspot activity.

My claim that the hotspots provide the driving force for plate motions is based on the following observations to be discussed below. (1) Almost all of the hotspots are near rise crests and there is a hotspot near each of the ridge triple junctions, agreeing with the notion that asthenosphere currents are pushing the plates away from the rises. (2) There is evidence that hotspots become active before continents split apart. (3) The gravity pattern and regionally high topography around each hotspot suggest that more than just surface volcanism is involved at each hotspot. (4) Neither rises nor trenches seem capable of driving the plates.

The symmetric magnetic pattern and the "mid-ocean" position of the rises indicate that the rises are passive. If two plates are pulled apart, they split along some line of weakness and in response asthenosphere rises to fill the void. With further pulling of the plates, the laws of heat conduction and the temperature dependence of strength dictate that future cracks appear down the centre of the previous "dike" injection. If the two plates are displaced equally in opposite directions or if only one plate is moved and the other held fixed, perfect symmetry of the magnetic pattern will be generated. The axis of the ridge must be free to migrate (as shown by the near closure of rises around Africa and Antarctica). If the "dikes" on the ridge axis are required to push the plates apart, it is not clear how the symmetric character of the rises could be maintained. The best argument against the sinking lithospheric plates providing the main motive force is that small trench-bounded plates such as the Cocos plate do not move faster than the large Pacific plate⁸. Also, the slow compressive systems would not appear to have the ability to pull other plates away from other units. The pull of the sinking plate is needed to explain the gravity minimum and topographic deep locally associated with the trench system⁹, but I do not wish to invoke this pull as the principal tectonic stress. This leaves sub-lithospheric currents in the mantle and the question now is: are these currents great rolls (mirrors of the rise and trench systems), or are they localized upwellings (that is, hotspots)?

A recent world gravity map¹⁰ computed for spherical harmonics up to order 16 shows isolated gravity highs over Iceland, Hawaii, and most of the other hotspots. Such gravity highs are symptomatic of rising currents in the mantle. Even if the gravity measurements are inaccurate (different authors have very different gravity maps), the fact remains that the hotspots are associated with abnormally shallow parts of the oceans. For example, note the depth of the million square kilometres surrounding the Iceland, Juan de Fuca, Galapagos, and Prince Edward hotspots. The magnitude of the gravity and topographic effect should measure the size of the mantle flow at each hotspot.

There is evidence of continental expression of hotspot activity in the lands bordering the Atlantic: the Jurassic volcanics in Patagonia (formed by the present day Bouvet Island plume), the ring dike complex of South-west Africa and flood basalts in the Parana Basin (Tristan da Cunha plume), the White Mountain Magma series in New Hampshire (the same hotspot that made the New England Seamount Chain (Azores plume?)), the Skaegaard and the Scottish Tertiary Volcanic Province (Iceland plume) and perhaps others. I claim this line-up of hotspots produced currents in the asthenosphere which caused the continental break-up leading to the formation of the Atlantic. Likewise the Deccan Traps (Reunion plume) were symptomatic of the forthcoming Indian Ocean

ripping. A search should be made for such continental activity, particularly in East Africa and the western United States (the Snake River basalts?) as an explanation for the rift features found there. There is a paucity of continental hotspots in Fig. 1; perhaps this is a bias due to continental complexity versus oceanic simplicity, but the model presented here predicts that most hotspots will be near a spreading rise.

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Fossil Pingos in the South of Ireland

REMAINS of former pingos or ice-lens mounds are known in the Low Countries, Scandinavia, East Anglia¹ and Wales². They are also widely distributed in the south of Ireland.

Fine examples occur near Camaross, on the road from Wexford to New Ross, 9 km west-north-west of Wexford town. A cluster of at least twenty fossil pingos north-east of Camaross Cross Roads (S891249) have been specially photographed by Dr J. K. St Joseph, Director in Aerial Photography, University of Cambridge (obliques 23/890245 and 7; verticals K17/W25-30). Part of K17/W26 is reproduced here by kind permission (Fig. 1a).

Three fossil pingos are seen. Pingo A (top right) has a basin about 60 m in diameter; the enclosing rim, which is almost complete, rises in a grassy ridge 1.5 m above the surrounding badly drained rushy ground (well seen on the south-east side). The elongated basin of pingo B does not contain open water, but the rushy vegetation inside the rim and outside it on the south-east is in contrast with the grass cover of the rim. Pingo C (bottom left) has an elongated basin which contains a wet swamp. In this vicinity, ridges of rock at about 75 m OD (ordinance datum) separate small valleys whose lower slopes (falling from north-west to south-east) are covered by soliflucted till with very poor natural drainage. The pingos formed near the base of the slopes.

Fine examples also occur at 60 m OD in Carrigeenhill Townland (W945953) on the road from Fermoy to Tallow, 14 km east-south-east of Fermoy. Here there is a rock-ridge to the north of the road, and a small valley partly filled with badly drained soliflucted till to the south. A large field abutting against the south side of the road showed at least three pingo basins and several curved ridges. When seen, the field was in the course of being drained and the lay-out of the drainage ditches had been imposed by the distribution of the pingo basins and ridges, which had disturbed the solifluction slope.

About 5 km west-north-west of Castleisland, the road from Castleisland to Tralee runs through a series of groups of fossil

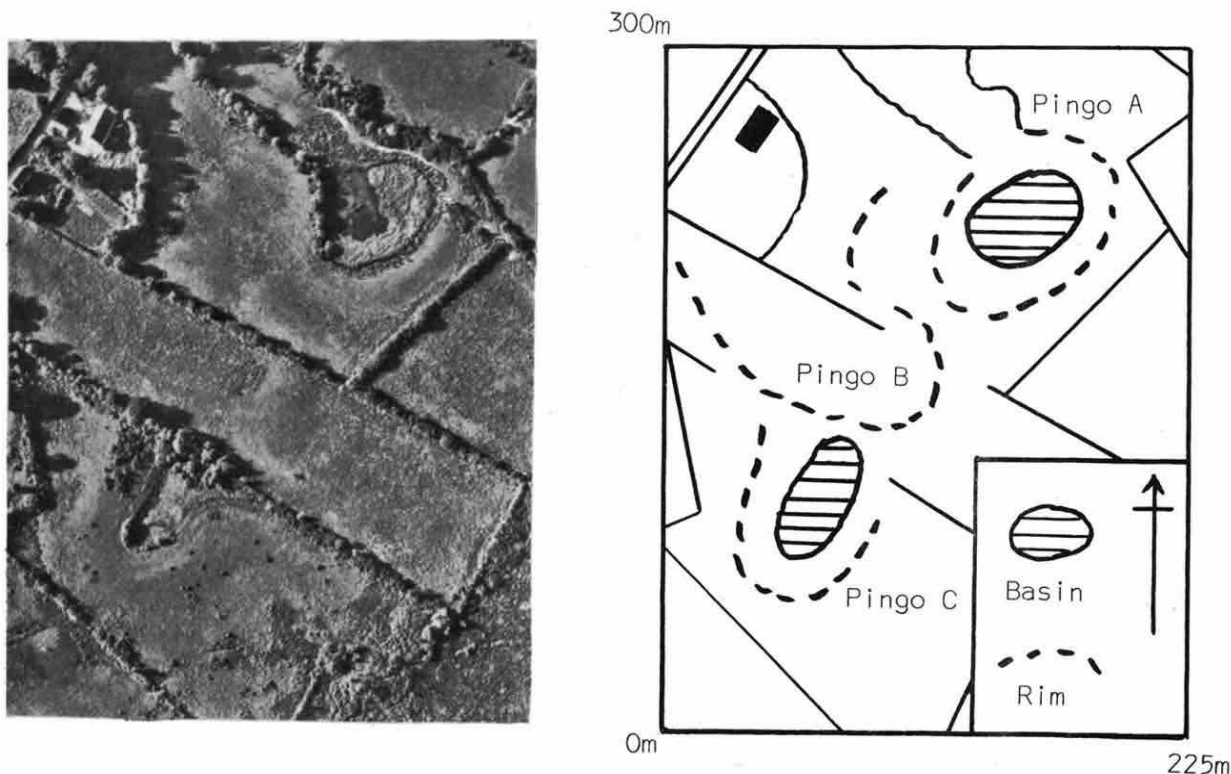


Fig. 1 a, Vertical aerial photograph (Cambridge K17/W26; reproduced by kind permission) of three fossil pings in Camaross Townland, Co. Wexford, Ireland. b, Outline map to accompany aerial photograph.

pingos at about 35 m OD. In Ballyegan Townland (Q962113) a small badly drained hollow is surrounded by outcrops of limestone; active quarrying is going on immediately to the west. There are at least three crescent shaped pingo ridges in the basin; one has an arc of 180° and a height of 1.5 m. In the adjoining Coolgarriv Townland (Q950117) there is a large marshy area, sloping gently to a flat valley bottom drained by the River Maine, with pingo features on both sides of the road. The road traverses one large pingo with appreciable rises where it crosses the rims which are about 100 m apart. South of the road, an almost continuous rim with an arc of 180° can be seen. In the next adjoining Maglass and Maglass East Townlands (Q945118) there are pingo features on both sides of the road.

These examples are only a few noted from main roads; many more will be revealed by survey. I will be obliged for information of other pingo features in Ireland. So far, all the examples noted lie south of the limit of the last glaciation.

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Determination of Hydrocarbons in Seawater Extracts of Crude Oil and Crude Oil Fractions

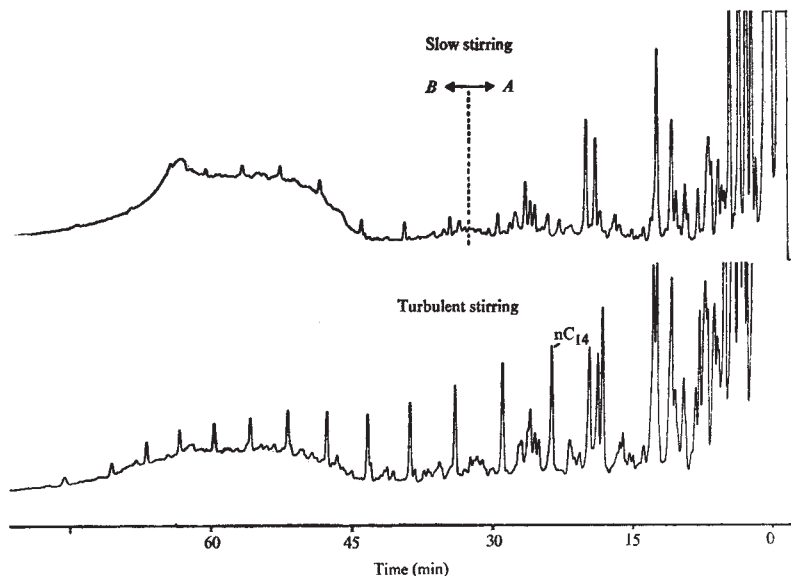
In spite of very convincing evidence to the contrary, crude oil and oil products are still occasionally considered to be "insoluble" in aqueous systems. In view of recent reports of major ecological disruptions caused by oil pollution^{1,2}, it

seemed imperative to collect more data on the solubility of oil and oil products in water systems^{3,4}. We have isolated, identified and quantitatively determined major components present in seawater extracts of several crude oils and a kerosene.

Oil samples (25 ml.) were mixed with filtered seawater (1.5 l., Whatman No. 1 filter paper) in different ways to simulate natural process of contact between oil and water. Slow stirring (12 h) consisted of stirring with a magnetic stirrer (4 cm $\times$ 1 cm 'Teflon' coated) at a rate that produced an oil vortex which was never greater than 25% of the solution depth. Turbulent conditions were effected by rapid stirring (12 h) to produce contact between the oil and the stirring bar. All stirred samples were processed in glass stoppered bottles (2 l., 12 cm in diameter) equipped with bottom taps. Maximum mixing was accomplished by shaking the oil/water samples in an Erlenmeyer flask (2 l.) using the Burrell wrist action shaker. All experiments were conducted at 23° C. Processed samples were transferred carefully to separatory funnels (2 l.) and allowed to stand for 2 h before the two phases were separated. The aqueous phase was then extracted four times with doubly distilled pentane. The evaporated residue from the pentane extract was chromatographed on a Varian Aerograph-1200 gas chromatograph interfaced to a CEC-104 mass spectrometer⁵. Component structures were assigned on the basis of mass spectra, boiling point comparisons⁶ and ultraviolet spectra. Concentration of dissolved compounds was determined by planimetry of gas chromatographic peaks.

Kuwait oil and a kerosene were stirred slowly, stirred turbulently and shaken with seawater. In general, slow stirring gave a reproducible aqueous extract that contained mostly aromatic (benzenes and naphthalenes in the low boiling range 80 – 262° C) compounds. As turbulence increased, however, more non-polar saturated hydrocarbons appeared in the seawater layer. Fig. 1 shows a gas chromatogram of the seawater extracts of Kuwait oil in conditions of slow

Fig. 1 Gas chromatograms of seawater extracts of Kuwait crude oil. Slow stirring conditions: *A*, benzene and naphthalene regions; *B*, polar aromatic region. Turbulent stirring conditions: increase in chromatogram complexity is due to presence of saturated hydrocarbons. The size of the column was 1/16 inch $\times$ 8 feet; the coating was 0.2% 'Apiezon L' on textured glass beads (80–100 mesh); temperature was 60°–220° C (4°/min); the carrier gas passed at 10 ml/min, and the chart speed was 15 inches/h.



and turbulent stirring. There was an increased concentration of saturated hydrocarbons in the aqueous extract when the mixture was stirred turbulently. The presence of saturated hydrocarbons in this extract was probably due to oil droplet suspension and not to true water solubility. Oil samples shaken with seawater produced a water phase (oil emulsion) with an oil content which was similar to that of the untreated oil.

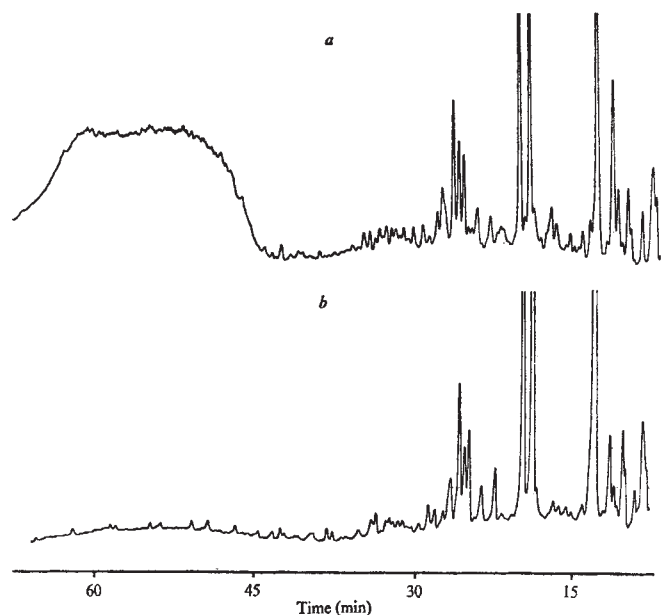


Fig. 2 Comparison of gas chromatograms of seawater extracts of Kuwait (*a*) and Louisiana (*b*) crude oil (slow stirring conditions). Specifications as in Fig. 1.

The high boiling envelope in Kuwait aqueous extract (Fig. 1*B*) is due mostly to polar aromatic material. This was established through gas chromatography of the aqueous extract on active silica gel. Polar solvents (25% $\text{CH}_3\text{OH}/\text{CH}_2\text{Cl}_2$) were needed to elute the bulk of this material. Fig. 2 compares the gas chromatogram of the Louisiana crude aqueous extract with that of the Kuwait extract. Although there are subtle differences in the naphthalene and benzene regions, the principal difference is in the intensity of the high boiling polar aromatic envelope.

The kerosene seawater extract (Fig. 3) contained a benzene and naphthalene distribution similar to the Kuwait extract,

but concentrations of dissolved compounds varied markedly (Figs. 1 and 3; Tables 1 and 2). One of the peaks (No. 15, Fig. 3) is only found in significant amounts in the kerosene fraction and has been identified as biphenyl. The concentrations reported in Tables 1 and 2 represent the distributions of crude oil and oil fractions in seawater (equilibria conditions) and do not represent true solubilities of those compounds in seawater.

Methods are being sought for rapid identification of oil that will prove useful in the enforcement of oil pollution legislation. Gas chromatography of aqueous extracts of polluting oils provides an excellent, sensitive method for this purpose. In each oil or kerosene studied, significant differences in relative amounts of naphthalene compounds were observed (Fig. 4). Special attention should be drawn to relative isomer content, that is, 1 and 2-methyl naphthalenes and the 1 : 2, 1 : 6, 2 : 6-dimethyl naphthalenes of each sample. Because each isomeric aromatic compound has approximately

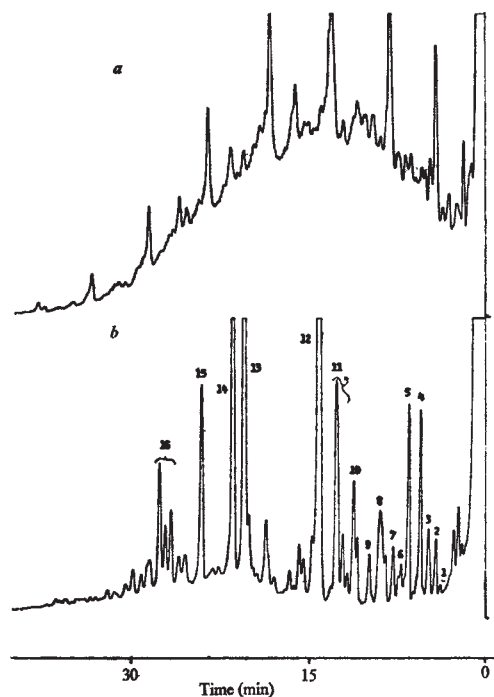


Fig. 3 Gas chromatograms of: *a*, kerosene; *b*, seawater extract of kerosene. Numbered peaks are identified and quantified in Table 1. Specifications as in Fig. 1.

Table 1 Substances isolated from Seawater Extract of Kerosene

COMPOUND		C B.P.T.	PEAK #	CONC. ($\mu\text{G/L}$)
BENZENES				
1-METHYL-2-ETHYL BENZENE		161.3	2	6.68
1-METHYL-4-ETHYL BENZENE		161.99	2	
1:3:5-TRIMETHYL BENZENE		164.72	3	8.10
1-METHYL-2-ETHYL BENZENE		165.15	3	
1:2:4-TRIMETHYL BENZENE		169.35	4	20.15
1:2:3-TRIMETHYL BENZENE		176.08	5	18.07
			6	
1-METHYL-4-PROPYL BENZENE		183.30	7	5.93
1-METHYL-2-PROPYL BENZENE		184.80	7	
1:3-DIMETHYL-5-ETHYL BENZENE		183.58	7	18.17
1:3-DIMETHYL-4-ETHYL BENZENE		188.20	8	
1:2-DIMETHYL-4-ETHYL BENZENE		189.40	8	
1:3-DIMETHYL-2-ETHYL BENZENE		190.01	8	
1-METHYLINDANE		190.6	8	
2-METHYLINDANE		191.4	8	6.21
1:3-DIMETHYL-2-ETHYL BENZENE		190.1	8	
1:2-DIMETHYL-3-ETHYL BENZENE		193.91	9	16.57
1:2:3:5-TETRAMETHYL BENZENE		198.00	10	
1:2:4:5-TETRAMETHYL BENZENE		196.80	10	35.39
4-METHYL INDANE		205.5	11	
1:2:3:4-TETRAMETHYL BENZENE		205.04	11	27.49
1:3-DIMETHYL-4-PROPYL BENZENE		206.6	11	
1:2-DIMETHYL-4-PROPYL BENZENE		208.5	11	32.95
TETRAHYDRONAPHTHALENE		207.57	11	
NAPHTHALENES				
NAPHTHALENE		217.96	12	152.89
2-METHYL NAPHTHALENE		241.05	13	85.66
1-METHYL NAPHTHALENE		244.64	14	63.43
BIPHENYL		255.2	15	27.49
1:2-DIMETHYL NAPHTHALENE			16	32.95
1:6-DIMETHYL NAPHTHALENE			16	
2:6-DIMETHYL NAPHTHALENE		262	16	
TOTAL AROMATICS				610

the same physical property and is not easily attacked by bacteria, one would expect concentration ratios of isomeric groups to remain constant even after exposure to air or water. This technique is useful as long as aromatic constituents are still present in the contaminating oil. Specific compounds such as biphenyl (kerosene aqueous extract, Fig. 3, No. 15, and Fig. 4) will also aid in the identification of oil.

Petroleum and petroleum products are toxic to fish⁷⁻⁹. Unfortunately, the toxicity values reported are meaningless except as an initial screening estimate¹⁰. If we can assume that the relative lethal concentrations reported (Table 3) give a general idea of the toxicity index of oil and oil fractions in the polluted environments, then naphthalene and probably other naphthalene-type compounds are the most toxic components identified in the aqueous extracts. The greater toxicity of kerosene than Kuwait crude oil to fish is supported by the detection of much greater concentrations of naphthalene in the kerosene seawater extract (kerosene extract—159.9 $\mu\text{g/L}$, Kuwait crude extract—32.3 $\mu\text{g/L}$). Thus by analysing seawater extracts it should be possible to establish a toxicity index of potential oil pollutants.

Changes in the behaviour of marine organisms, such as disruption in normal feeding sequences, sex attraction, host recognition in symbiotic relations and other intricate social interactions, may be caused by the presence of sublethal concentrations of soluble oil fractions. Because chemical communication among marine organisms is expected to be very susceptible to interferences by pollution the effects of oil on such systems are being investigated in this laboratory.

The presence of a high boiling aromatic envelope (Fig. 1B, Table 2) in the aqueous extract of Kuwait oil is most interesting. Such water soluble materials, when concentrated and retained by marine animals¹¹, could present a health hazard. For this

Table 2 Relative Concentration of Aromatic Species in Kuwait Oil Seawater Extracts (Slow Stirring)

	$\mu\text{g/L}$	Concentration
Benzenes and naphthalenes (Fig. 1A)	657	45.2%
Polar aromatics (Fig. 1B)	796	54.8%
Total amount of oil in water	1,453	
Concentration of individual compounds		
Naphthalene	32.3	(2.2%)
2-Methyl naphthalene	15.0	(1.0%)
1-Methyl naphthalene	18.0	(1.2%)

Table 3 Toxicity Data

Oil fraction	General toxicity range (mg/L)	Specific toxicity data
(1) Crude oil	5,000	1-100 mg/L.—pronounced toxic effect on larval stages of crabs and shrimp
(2) Bunker C (No. 6 fuel oil)	2,400*	
(3) Diesel oil (No. 2 fuel oil)	200†	
(4) Kerosene (No. 1 fuel oil)	200†	
(5) Gasoline	90†	
(6) Benzene, toluene	10-90‡	386-1,180 mg/L.—mosquito fish (TLm—96 h)
(7) Naphthalene	4-5‡	1.8-3.2 mg/L.—fingerling salmon (death—72 h)
		150 mg/L.—mosquito fish (TLm—96 h)
		20-40 mg/L.—perch (death—1 h)
(8) Phenanthrene	4-5‡	
(9) Phenols	1-20‡	

These reported toxicity ranges have been determined under laboratory conditions and are not quantitatively valid for the natural environment.

TLm, Concentration at which 50% of the aquatic animals survive.

* Juvenile shad (TLm—48 h).

† Juvenile shad (TLm—24 h).

‡ Sunfish (death—1 h).

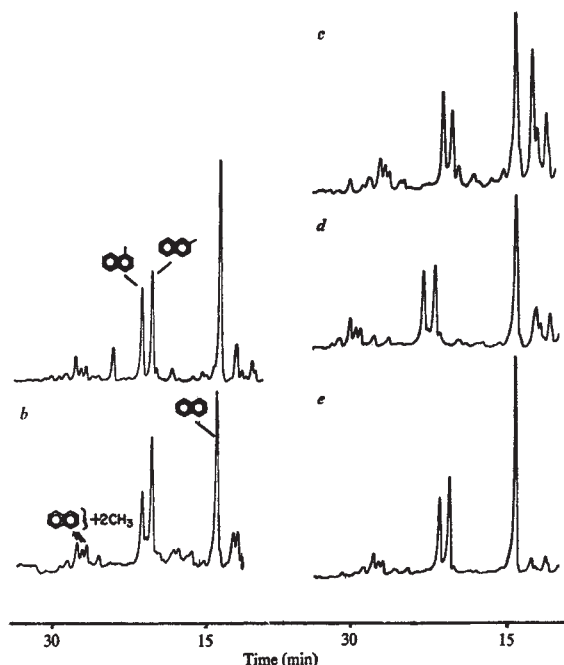


Fig. 4 Gas chromatograms of seawater extracts of: *a*, kerosene (Gulf grade); *b*, No. 6 fuel oils (North-East Petroleum Co.); *c*, Kuwait crude oil; *d*, La Rosa crude oil; *e*, Louisiana crude oil. Specifications as in Fig. 1.

reason, future investigations will concern the identification of water soluble components in the high boiling region.

We thank Dr Max Blumer for discussions and help. Samples of No. 2 and No. 6 fuel oils were obtained from Mr J. L. Cronin (North East Petroleum Corporation) and the crude oil samples were provided by G. P. Canevari (Esso Research and Engineering Company). This work was supported financially by the Federal Water Quality Administration.

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Stratospheric Warming observed by Nimbus 4

An important feature of the winter stratosphere near the poles is the occurrence of "sudden warmings" where the temperature over a small area may rise by as much as 50 K in a few days, accompanied by a major change in circulation. Observation of these events, dependent on meteorological rocket and radiosonde ascents, has been hampered by the

sparsity of data. Here we present data for a stratospheric warming obtained by the selective chopper radiometer on the Nimbus 4 satellite, which gives global coverage to considerably greater heights than has hitherto been possible.

The radiometer¹⁻³ has six channels, each accepting infrared radiation emitted by carbon dioxide in the atmosphere below the satellite at wavelengths of approximately 15 μ m. The radiation measured by channel A originates in a layer approximately 20 km thick centred at the 2 mbar (42 km) level, while that measured by channel B originates in a layer of similar thickness centred at 20 mbar (26 km). Figs. 1*a* and *c* are northern hemisphere maps of channel A radiance, while Figs. 1*b* and *d* are northern hemisphere maps of channel B radiance. Such maps are produced routinely by 1500 h on the day following the day of the observations. A table is given of the black body temperature equivalent to infrared radiance for channels A and B. The interpretation in terms of atmospheric temperature is explained in ref. 3; as a rough guide it may be considered that these temperatures are those occurring in the region of the 2 mbar and 20 mbar levels respectively.

Figs. 1*a* and *b* show the situation on December 30, 1970. On channel A (Fig. 1*a*) a warm region with a maximum radiance of 95 $\text{mW m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$ was developing at 45° N 65° E; 3 days earlier this warm region had been at 25° N 20° E with a maximum radiance of 85 $\text{mW m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$. A corresponding warm region was present on channel B (Fig. 1*b*) with a maximum at 35° N 50° E, implying a northward tilt of the warm anomaly.

Table 1 Equivalent Temperature as a Function of Radiance for a Black Body

Radiance $\text{mW m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$	Equivalent temperature (K)
30	201.0
40	213.7
50	224.7
60	234.5
70	243.6
80	251.9
90	259.8
100	267.2
110	274.2
120	281.0

By January 4, 1971, a great change had occurred in the temperature distributions observed by both channels. The warm area described above had intensified and at the 2 mbar level (Fig. 1*c*) had moved north to 70° N 90° E with a radiance at the centre of 120 $\text{mW m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$. At this time the difference in equivalent temperature near the 2 mbar level between the hottest and coldest region was more than 60 K. The whole northern hemisphere was dominated by a wave number one pattern with little contribution from the higher wave numbers apparent on December 30. The pattern was similar at the 20 mbar level but the amplitude of wave number one was less and the phase is about 25° farther eastward. This implies a westward tilt of the warm anomaly of 1.5° longitude per km height. Subsequently the warm region increased little in intensity, but moved westward. On January 9, the warm region had moved to 75° N 40° E with a westward slope of 3° longitude km^{-1} ; thereafter the warm region at the 2 mbar level moved farther westwards and decayed in intensity.

On channel A on December 30 (Fig. 1*a*), there was a warm region at 65° N 120° E and a corresponding warm region on channel B (Fig. 1*b*) at 65° N 170° E; this was what remained of a warming which had a maximum intensity on December 26 at 70° N 125° E at the 2 mbar level. On December 30 this warm anomaly had a westward tilt of 3° longitude km^{-1} while the cold anomaly over North America had a westward tilt of approximately 4.5° longitude km^{-1} . By January 2 this warm anomaly had disappeared from our maps.

The increase of amplitude of the warming and the westward phase tilt with height are to be expected if temperature varia-

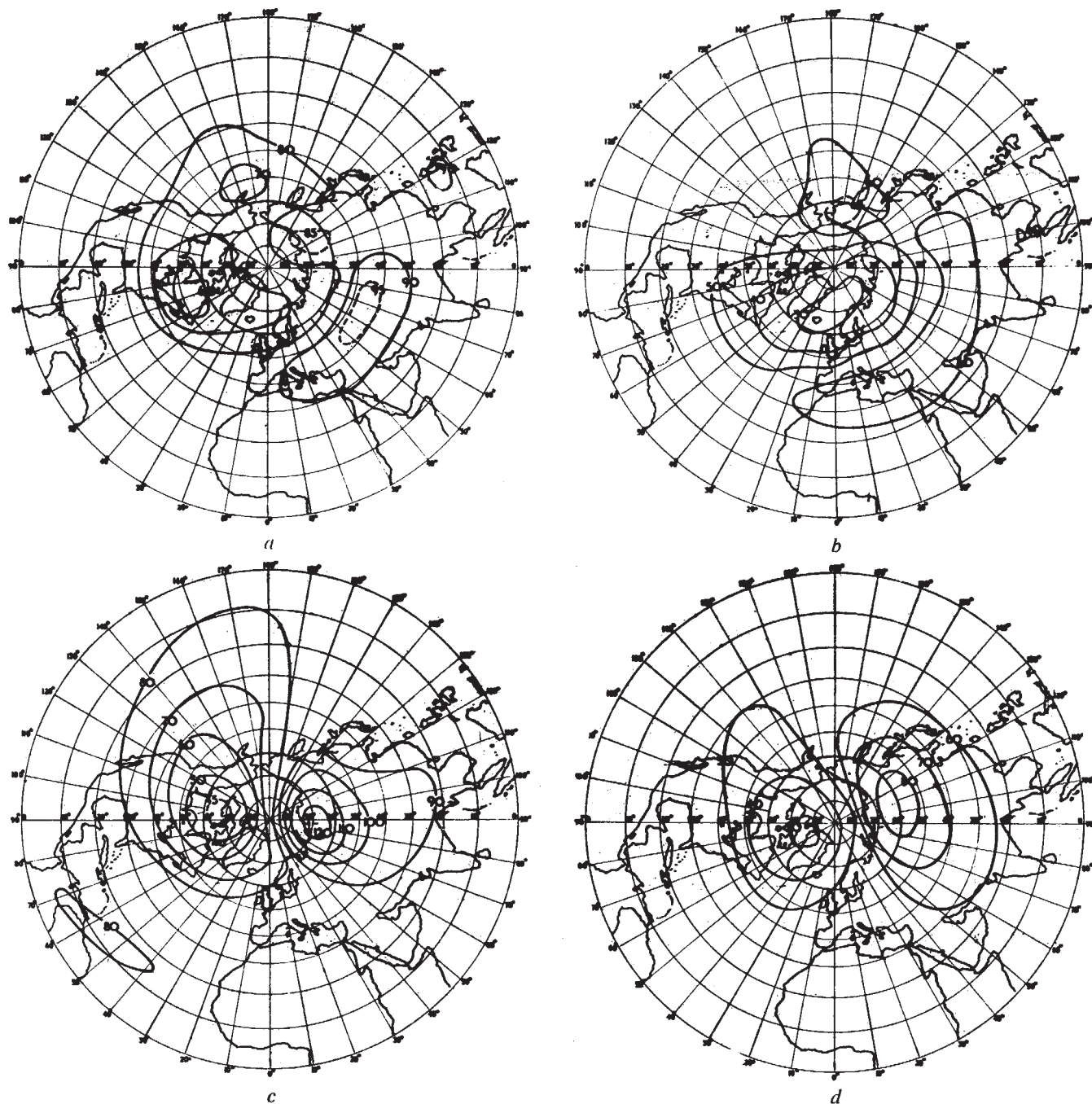


Fig. 1 North Polar stereographic maps showing isopleths of radiance $\text{mW m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$ from channels A and B. The equivalent temperatures of a black body which would give the same radiance are given in Table 1. Each map was derived from data of 12 orbits extending to 80°N (ref. 3). a, Channel A (approximately 2 mbar), December 30, 1970; b, Channel B (approximately 20 mbar), December 30, 1970; c, Channel A, January 4, 1971; d, Channel B, January 4, 1971.

tions are caused by upward propagating planetary waves^{4,5}.

The selective chopper radiometer was built under a Science Research Council grant to Oxford and Reading Universities and was mounted on the NASA Nimbus 4 satellite launched on April 8, 1970. Data from the satellite have been transmitted to the UK by NASA on a near real time basis. We acknowledge the support and cooperation of NASA.

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BIOLOGICAL SCIENCES

Neonatally Induced Allograft Tolerance may be mediated by Serum-borne Factors

IN 1953, Billingham *et al.* discovered that mice inoculated neonatally with foreign lymphoid cells can often accept skin grafts from the same foreign strain, indicating that they have become "tolerant" to the alloantigens of the neonatally inoculated cells^{1,2}. A similar tolerance can be induced in adult animals if they are rendered chimaeric by inoculation with foreign bone marrow after either whole body irradiation or treatment with immunosuppressive drugs^{3,4}.

Many explanations can be offered for the establishment and maintenance of allograft tolerance, but two hypotheses only will be considered here. According to the first, lymphoid cells that would have been reactive to a given set of antigens become permanently depleted or inactivated in a tolerant animal⁵. In the second hypothesis the reason why lymphoid cells from tolerant animals are not reactive *in vivo* is that they are temporarily suppressed by some factor which is present in the tissues or serum of the tolerant animals, for example, "tolerogenic" antigen or enhancing antibodies, which would act either on the lymphocytes or on the target cells.

We now report preliminary findings from a study using a cytotoxic test to ascertain whether mice neonatally made tolerant to allografts have lymphocyte-mediated immunity (detectable *in vitro*) against target cells of the strain to which they were tolerant *in vivo*, and whether serum from the tolerant animals can abolish such a manifestation of immunity.

Ten CBA and ten A mice were rendered tolerant by injecting within the first 24 hours after birth with 5×10^7 spleen cells of A $\times$ CBA F1², tolerance being defined as the ability of approximately one month old mice to accept permanently skin grafts of A and CBA origin, respectively. Lymph nodes (axillary, inguinal, cervical and retroperitoneal) and serum were obtained from four 1–2 months old CBA mice which were tolerant to A, and from five A mice tolerant to CBA, each mouse being studied separately. The litter mates of these mice, which had also been injected with spleen cells neonatally, were shown to be tolerant by skin grafting tests; the mice used in the cytotoxic assays had not, however, received any skin grafts. Lymph nodes and serum from CBA and A mice which had not been injected with foreign cells were used as controls.

CBA and A fibroblasts derived from explanted adult lung tissue were plated *in vitro* and exposed to serum from specifically tolerant and control mice, after which lymph node cell (LNC) suspensions from the same mice were added. Instead of the colony inhibition technique, used previously⁶, we used a modification of a similar test⁷ in which the cytotoxic effect of immune lymphocytes is measured on all the cells in microcultures rather than on their ability to inhibit colony formation; smaller samples of lymphocytes and sera could therefore be used. Cultures of CBA and A fibroblasts, derived from the lungs of adult animals, were trypsinized, and 80–100 cells, suspended in Waymouth's culture medium containing 30% newborn calf serum, were seeded into each microwell of flat-bottomed 3040 Falcon Microtest II plates. On the next day the medium was removed and the sera to be tested were diluted 1 : 6 in Waymouth's medium and 0.1 ml. was added to each well, after which the plates were incubated for 45 min. The sera were then poured off, and LNC suspensions added, 0.2 ml. per well, each well receiving 400,000 LNC suspended in Eagle's minimum essential medium (MEM). The plates were incubated for an additional 45 min, and then 0.05 ml. of MEM containing 50% newborn calf serum was added to each well. The plates were covered with Falcon 3044 pressure-sensitive film, the lids were put on and the plates incubated at 37° C for 2–3 days on a Bellco rocker platform in an atmosphere of 5% CO₂ in air. The plates were then stained with crystal violet and the cells in

each well counted, using coded material. Each determination was made on 8–11 wells treated identically.

The findings are presented in Table 1. It seems that LNC from

Table 1 Data from Cytotoxicity Tests with Explanted Lung Fibroblasts from CBA and A Mice, exposed to Lymph Node Cells (LNC) and Sera from Tolerant and from Control Animals

Experi- ment No.	Tar- get cells	LNC	Serum	No. of cells per well (mean ± s.e.)	% Decrease in cell No./ well with tolerant LNC and control as compared to tolerant serum
1-a	A	CBA tolerant	CBA tolerant	155.2 ± 6.4	24.0
			CBA normal	117.9 ± 2.7 †	
		CBA tolerant	CBA tolerant	152.3 ± 4.8	
1-b	CBA	A tolerant	A tolerant	153.9 ± 7.0	35.6
			A normal	52.5 ± 3.2	
			—	33.8 ± 3.3 ‡	
2-a	A	CBA normal	A normal	28.3 ± 2.9	38.8
		CBA tolerant	—	46.3 ± 4.2	
		CBA tolerant	CBA tolerant	36.5 ± 2.5	
2-b	CBA	A tolerant	A tolerant	34.5 ± 1.5	38.3
			A normal	21.1 ± 1.2 ‡	
			—	24.3 ± 1.9	
3-a	A	CBA tolerant	—	32.4 ± 1.2	23.4
		CBA tolerant	CBA normal	37.9 ± 1.7	
			A normal	36.4 ± 1.9	
3-b	CBA	A tolerant	A tolerant	16.7 ± 0.9	28.8
			A normal	10.3 ± 0.5 ‡	
			—	8.9 ± 0.9	
4-a	A	CBA tolerant	—	15.6 ± 1.1	20.1
		CBA tolerant	CBA normal	16.5 ± 0.9	
			A normal	19.3 ± 1.4	
4-b	CBA	A tolerant	A tolerant	18.8 ± 1.3	19.6
			A normal	52.5 ± 3.7	
			—	40.2 ± 2.9 *	
5-a	A	CBA tolerant	CBA tolerant	35.0 ± 1.7 †	25.5
			CBA normal	39.1 ± 2.3 †	
			A normal	20.8 ± 1.9	
5-b	CBA	A tolerant	A tolerant	14.8 ± 1.3	28.8
			A normal	16.2 ± 0.9 *	
			—	14.9 ± 1.3 *	
6-a	A	CBA tolerant	CBA tolerant	150.9 ± 5.8	20.1
			CBA normal	120.7 ± 3.8 ‡	
			A normal	126.2 ± 3.2 ‡	
6-b	CBA	A tolerant	A tolerant	157.6 ± 7.7	16.4
			A normal	149.8 ± 6.9	
			—	156.5 ± 9.0	
7-a	A	CBA tolerant	CBA tolerant	165.3 ± 7.4	28.4
			CBA normal	153.8 ± 9.1	
			A normal	14.8 ± 1.3	
7-b	CBA	A tolerant	A tolerant	10.6 ± 0.7 †	19.6
			A normal	11.9 ± 0.5 *	
			—	11.9 ± 0.5 *	
8-a	A	CBA tolerant	CBA tolerant	15.2 ± 0.8	20.1
			CBA normal	20.3 ± 2.0	
			A normal	19.2 ± 1.1	
8-b	CBA	A tolerant	A tolerant	20.1 ± 1.2	28.4
			CBA normal	18.2 ± 1.5	
			A normal	14.4 ± 0.9	
9-a	A	CBA tolerant	CBA tolerant	9.3 ± 1.1	35.4
			CBA normal	7.8 ± 0.5	
			—	8.5 ± 1.2	
9-b	CBA	A tolerant	A tolerant	19.9 ± 1.7	45.8
			A normal	18.9 ± 1.9	
			—	18.8 ± 1.5	
10-a	A	CBA tolerant	CBA tolerant	15.9 ± 1.8	20.1
			CBA normal	48.0 ± 2.5	
			A normal	50.6 ± 3.3	
10-b	CBA	A tolerant	A tolerant	46.9 ± 2.1	40.6
			CBA normal	40.6 ± 0.9	
			—	40.6 ± 0.9	

The target cells were exposed to sera from specifically tolerant and normal untreated mice; CBA tolerant = CBA mouse tolerant to A; A tolerant refers to a mouse tolerant to CBA; A normal and CBA normal refer to normal untreated A and CBA mice. Between eight and eleven microwells were included in each group.

Percentage decrease in cell number per microwell with LNC from specifically tolerant donors in the presence of serum from control as compared to the tolerant donors was calculated. Probabilities that the differences between groups receiving control (as compared to "tolerant") serum were due to chance were calculated: * $P=0.05$; † $P=0.01$; ‡ $P=0.001$.

CBA mice tolerant to A grafts, were cytotoxic to cultivated A fibroblasts in the presence of control sera, and that serum from the tolerant animals could nullify that effect. Reciprocal findings were obtained with tolerant A mice. The effect obtained with LNC and sera from the tolerant mice was specific: serum from CBA mice tolerant to A protected cultivated A target cells but did not protect cultivated CBA target cells and vice versa. No abrogation of LNC-mediated immunity was found when serum derived from the strain of target cell origin was added to the cultures. Lymph node cells from tolerant mice had a slight inhibitory effect to syngeneic target cells in experiments 1 and 2-a (as compared with LNC from non-tolerant donors), but did not have any such effect in experiments 2-b and 5.

Although the degree of cytotoxicity detected with LNC from tolerant donors was less than in previous experiments⁷⁻⁹, the consistency in the data obtained makes it unlikely that they were due to chance. These findings are best explained on the basis of the second hypothesis, according to which the mice were tolerant because they contained serum factors capable of inducing and maintaining the tolerant state. As serum from the mouse strain from which target cells were derived gave no protection, the tolerant state is not likely to be maintained by the circulation of soluble alloantigens capable of binding to otherwise reactive lymphocytes. Such antigens should be present also in mice of the respective target cell strains, unless the additional postulate is made that serum from mice rendered tolerant neonatally contains a special type of tolerogenic antigen. These observations do not make it possible to decide whether the factor abrogating the *in vitro* reactivity of the lymphocytes is an antibody or an antigen-antibody complex, or whether some other type of specific molecule is involved.

It could be argued that the fibroblasts used as target cells *in vitro* have a strain-specific antigen, which is lacking in the spleen cells used to induce "tolerance". In that case, however, the lymphocytes and sera from the "tolerant" mice would be expected to behave like those from normal mice, whereas the experimental results show that the former were presensitized.

Our findings are analogous to those obtained when studying radiation-induced canine chimaeras⁷, pregnant mice carrying the conceptus of an allogeneic mating⁸, or tumour-bearing individuals whose lymphocytes are immune to specific antigens of the same individuals' neoplastic cells, and who possess blocking serum factors which can specifically abolish the *in vitro* expression of the cell-mediated immunity⁹. Other evidence for the presence of antibodies in "tolerant" mice has been presented by Voison *et al.*¹⁰, who also conclude that this type of tolerance is not the result of induced unresponsiveness.

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Mouse Specific Bone Marrow-derived Lymphocyte Antigen as a Marker for Thymus-independent Lymphocytes

THE usefulness of the theta (θ) alloantigen as a marker for thymus-derived lymphocytes in mice¹⁻⁶ has emphasized the need for a distinguishing marker for thymus-independent (bone marrow-derived) lymphocytes, about which relatively little is known. We have raised an antiserum in a rabbit against lymph node cells from mice which had been thymectomized, irradiated and reconstituted with foetal liver cells, which after appropriate absorption was cytotoxic only to those lymphoid cells which were thymus-independent. Because the cell surface antigen(s) with which the antiserum is principally reacting seems to be species-specific, we have followed the example of Shigeno *et al.*⁷ and have called it MBLA (mouse-specific bone marrow-derived lymphocyte antigen).

CBA mice were thymectomized when 4-6 weeks old, lethally irradiated (950 r.) and reconstituted with 10^7 syngeneic foetal liver cells after 10-12 weeks. Six weeks later, 185×10^6 lymph node cells from twelve of these mice were injected intravenously into a rabbit, which was boosted intravenously after 1 week with 575×10^6 similar cells and bled out 1 week later. The serum was heat inactivated (56°C for 30 min) and absorbed with 0.1 volume liver, 0.1 volume packed red blood cells (RBC) and six times with 0.1 volume packed thymocytes. Each absorption was carried out at room temperature for 1 h with slow rotation with outbred mouse tissues except for the final two absorptions which were carried out with CBA thymocytes. The serum was tested by chromium-51 and dye exclusion cytotoxic testing using CBA target cells, hamster complement with thymocytes and guinea-pig complement with all other cell types as previously described⁸.

Before absorption the rabbit antiserum was of high titre (50% kill at 0.001 against spleen cells) and killed 100% of lymph node, spleen and thymus cells. After the final absorption the serum (now referred to as anti-MBLA) killed approximately 60% of spleen cells and 30% of lymph node cells, but it did not kill thymocytes and its cytotoxic activity for spleen cells could no longer be absorbed with thymus cells.

To establish that the antiserum was not killing thymus-derived lymphocytes, sequential cytotoxic testing was carried out. ⁵¹Cr-labelled lymph node cells were first killed with anti- θ (prepared as previously described¹) and complement, and the residual cells were then tested with anti-MBLA and vice versa. Lymph node cells remaining after lysis with anti- θ were no longer susceptible to anti- θ , but most of the cells could be killed by anti-MBLA (Fig. 1a). After lysis with anti-MBLA, the remaining cells were no longer susceptible to the same antiserum but the majority could be killed by anti- θ (Fig. 1b). The same result was obtained with ⁵¹Cr-labelled spleen cells. This established that the cytotoxic activities of anti- θ and anti-MBLA did not overlap. If all thymus-derived lymphocytes are θ -bearing^{1,2,8}, then anti-MBLA was not killing thymus-derived lymphocytes. It can be seen in Fig. 1 that a small but significant population of cells in lymph node was not killed by either anti- θ or anti-MBLA, although all the cells were killed by the unabsorbed rabbit antiserum. The nature of these resistant cells is unclear but they could be macrophages or their precursors, granulocytes, a third type of lymphocyte, or a mixture of these, with or without other cell types. As expected, the resistant cell population made up a

Table 1 Results of Dye Exclusion Cytotoxic Testing on CBA Lymphoid Tissues with Anti-MBLA, Anti- θ and Anti-MBLA+Anti- θ

Tissue *	Experiment number	Cytotoxic index † with		
		Anti-MBLA (%)	Anti- θ (%)	Anti-MBLA + anti- θ (%)
Thymus	1	0	>99	—
	2	0	>99	—
Blood	1	33	75	99
	2	34	68	90
Lymph node	1	30	58	86
	2	27	63	86
Spleen	1	58	29	78
	2	55	32	85
Peyer's patches	1	67	40	90
	2	69	31	86
Bone marrow	1	43	0	—
	2	36	0	—

* Cell suspensions were prepared as previously described⁶. More than 90% of the cells were lymphoid, except in bone marrow where 35–45% appeared to be lymphocytes.

† Cytotoxic index =

% dead cells with antiserum — % dead cells with normal serum (NMS)

100 — % dead cells with NMS

All sera were used neat or diluted 1:2, which for both anti-MBLA and anti- θ represented points on a cytotoxic plateau.

In some tests normal rabbit serum (absorbed in same manner as anti-MBLA) was used instead of NMS and gave identical results. In all tests the per cent dead cells with NMS was less than 12%.

larger proportion of the residual lymph node cells after initial lysis with anti- θ than with anti-MBLA (Fig. 1). Because approximately 70% of lymph node cells are θ -bearing⁸ compared with 30% MBLA-bearing (Table 1), initial lysis with anti- θ left a relatively small population of residual cells of which cells resistant to anti- θ and anti-MBLA made up a relatively large proportion. The reverse was true when the cells were first killed with anti-MBLA.

The proportion of MBLA-bearing cells in various lymphoid tissues of 3–4 month old CBA mice was studied using dye exclusion cytotoxic testing. As Table 1 shows, the distribution was inversely related to that of θ -bearing cells, and when anti- θ and anti-MBLA were used together they were additive. The fact that anti-MBLA was cytotoxic for spleen cells of 7/7 strains of mice (CBA, A, C57Bl/6, Balb/c, C3H, B10.D2, AKR) and was not cytotoxic for spleen cells of rats, guinea-pigs, hamsters or rabbits, suggests that MBLA is mouse-specific. In absorption studies with homogenates of mouse liver, brain and kidney, only liver absorbed any cytotoxic antibody and this absorption was minimal.

In preliminary experiments, anti-MBLA was found to be cytotoxic for MOPC-315 myeloma cells and when used with complement it inhibited most direct plaque-forming cells (D. W. Dresser and M. Raff—unpublished observations). It is therefore clear that MBLA is present on the surface of resting marrow-derived lymphocytes (antibody-forming cell precursors = AFCP) as well as on actively secreting antibody-forming cells (AFC), which we think develop from marrow-derived lymphocytes⁹. This is in contrast to the PC₁ alloantigen in mice which seems to be present only on AFC (ref. 10). A preliminary experiment showed that anti-MBLA inhibited 82%, anti- θ 22% and both together 97% of immune spleen rosette-forming cells against sheep RBC (M. F. Greaves and M. C. Raff, unpublished observations). It is unlikely that the cytotoxic or inhibitory activity of anti-MBLA is caused by anti-immunoglobulin antibodies, for the serum did not react in Ouchterlony testing with normal mouse serum (NMS) and its cytotoxic activity was unaffected by the presence of NMS. At the moment, the possibility that the antiserum kills a proportion of non-lymphoid cells (such as granulocytes and/or macrophages) as well as thymus-independent lymphocytes cannot be excluded.

The use of heteroantibodies to distinguish different types of lymphocytes reported here and by others^{11–13} suggests that

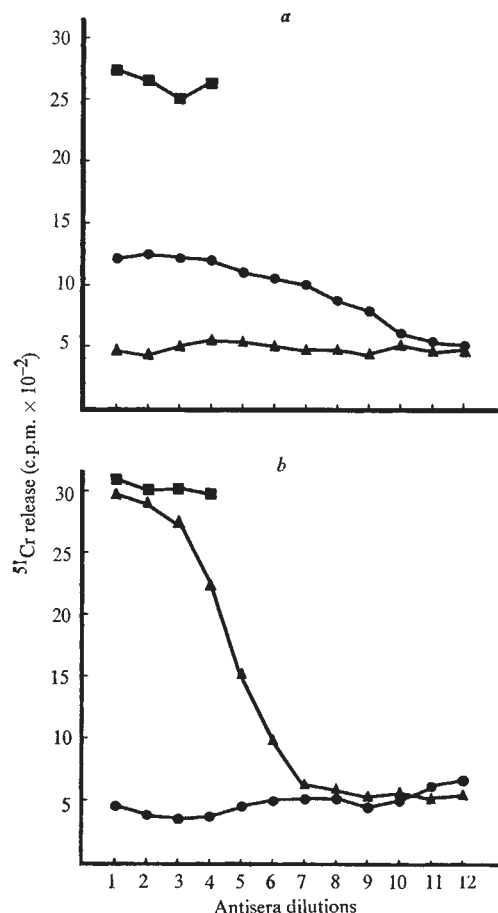


Fig. 1 Sequential ⁵¹Cr cytotoxic tests. CBA lymph node cells were labelled with ⁵¹Cr and killed with anti- θ and complement (a) or anti-MBLA and complement (b). [50×10^6 cells were incubated with 1 ml. of antiserum diluted 1:2 for 30 min at 37° C, washed and similarly incubated with 1 ml. of guinea pig complement diluted 1:3.] After washing, the residual cells were tested with anti- θ (▲), anti-MBLA (●) and the unabsorbed rabbit antiserum (■), with neat serum in the first tube in each case.

this approach may be fruitful in man. Besides its theoretical and diagnostic value, an anti-lymphocyte serum which was specific for human antibody-forming cells and their precursors could well have therapeutic application in view of the possible role of enhancing antibodies in tumours¹⁴.

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Quinacrine Fluorescence of the Human Y Chromosome

RECENT observations¹⁻⁴ of specific localized fluorescence in Y chromosomes stained with quinacrine mustard and quinacrine hydrochloride have provided a new parameter for the study of abnormal Y chromosomes. We have re-examined regularly prepared chromosome and buccal smear slides of at least 100 cells from two patients with a small Y chromosome⁵ and from patients with normal Y chromosomes. We used a modification of techniques described earlier¹⁻⁴. We stained for 5 min in 0.5% quinacrine hydrochloride (Winthrop); washed in distilled water; differentiated for 3 min in citric acid-phosphate buffer, pH 5.5; washed and mounted slides in phosphate buffer, pH 7.4, and sealed with nail polish. Preparations were examined with a Leitz Ortholux microscope with a mercury arc lamp, standard fluorescence filters, and dark-field illumination, and were photographed with Ektachrome high speed film. Fluorescence persisted for several days. Full details of the technique will be published later.

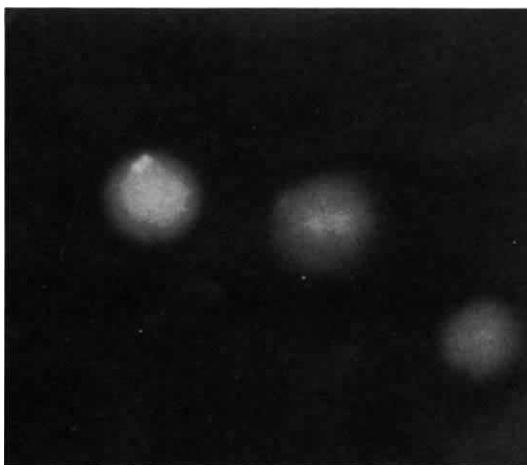


Fig. 1 Interphase nucleus showing two adjacent fluorescent bodies.

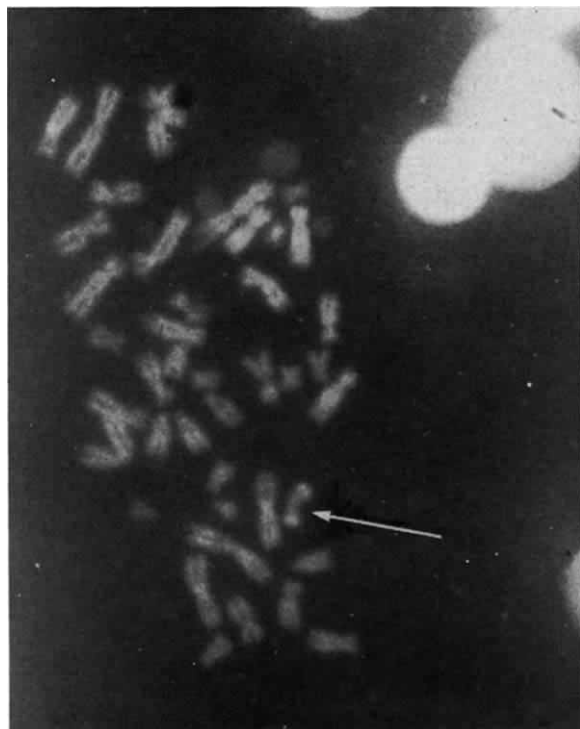


Fig. 2 Metaphase spread showing two fluorescent regions on the dicentric Y chromosome.

The distal portion of the long arm of the normal Y chromosome fluoresced clearly and distinctly in metaphase plates and a corresponding fluorescent particle was present in many interphase nuclei, but no comparable fluorescence was detected in preparations from patients with a small Y chromosome. This may be because of deletion of the heterochromatic region in the long arm.

We have also investigated a patient with a dicentric chromosome, and found two adjacent fluorescent spots in interphase nuclei (Fig. 1), and could see two fluorescent spots on both the distal ends of the dicentric Y chromosome (Fig. 2). It will be interesting to study patients with an iso-chromosome of the long arm of Y with this technique. One could expect to see two fluorescent regions in these patients.

We have previously shown that fertility and other sexual characteristics are normal in men with a small Y chromosome⁵. The observation described here indicates that maleness is not dependent on the presence of a fluorescent region in the long arm of the Y chromosome. Caution may therefore have to be exercised in using the quinacrine fluorescence technique for determination of the Y chromosome⁶.

We thank Mrs Ruth Classon and Miss Betsy Hill for technical assistance. We also thank Drs Hope Punnett and Kistenmacher, Children's Hospital, Philadelphia, for allowing us to study the slides of their patient with the dicentric Y chromosome. This work was supported in part by a grant from the US Public Health Service.

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Use of the Y Chromosome in Prenatal Sex Determination

REPEATED observations of the cellular composition of human endocervical mucus from the earliest weeks of pregnancy, using various staining techniques, have revealed not only cells of maternal origin, but also chorionic cells which have been shed into the fairly static cervical mucous plug.

I have used the fluorescein dye test¹⁻⁴ on smears taken with a cotton swab from the mid-cervical mucus of thirty patients in the first, second and third trimesters of pregnancy. In eighteen cases quinacrine hydrochloride revealed in a proportion of the interphase chorionic nuclei a single fluorescent spot, indicative of Y chromosome material, and thus of a male conceptus. In the other twelve patients smears developed no fluorescent spot, indicative of a female conceptus. Six of the eighteen have now been delivered of boys and four of the twelve have produced girls. These observations are being extended.

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Identification of Y and X Chromosomes in Amniotic Fluid Cells

A FLUORESCENT staining technique has been described¹⁻³ for identification of the Y chromosome in cells from various sources. The use of this technique to detect Y chromosomes in the amniotic fluid cells, in conjunction with a search for sex chromatin bodies in these cells, might provide a rapid and accurate means of prenatal sex determination.

Fresh amniotic fluid from twelve women whose pregnancies were being terminated was centrifuged at 800–1,000 r.p.m. for 20 min, the supernatant discarded and a smear of the cell pellet was prepared. Smears were fixed in methanol for at least 15 min, stained with a 0.5% aqueous solution of quinaque dihydrochloride for 5 min, washed in running tap water for 3 min and rinsed in 0.2 M acetate buffer (sodium acetate; glacial acetic acid), pH 5.5. The preparations were mounted in 1:1 glycerol and buffer. A minimum of 100 cells was examined from each specimen. In four specimens a fluorescent body (Y chromosome) was detected in 3–9% of the cells.

Sex chromatin was determined in eight specimens. One drop of the cell pellet of the amniotic fluid was mixed and stained immediately with one drop of 2% aceto-orcin and then compressed with a cover slip. Immediately after preparation the slides were examined for sex chromatin bodies. Two were sex chromatin positive, two were negative, four were unsatisfactory because there were too many pyknotic cells and too few cells suitable for analysis according to the selection criteria suggested by Hsu *et al.*⁴. All studies were done before expulsion of the fetuses. The results of Y fluorescence, sex chromatin and phenotypic sex determination for each abortus are shown in Table 1.

Pathological examination of the abortuses revealed five males and seven females. Although the four smears which developed fluorescent bodies were from males, one other male could not be detected by the method used. The reasons for our failure to detect a fluorescent body in the amniotic fluid cells are now being investigated. One possibility is that many of the cells were damaged or not viable, and this certainly seemed to account for the unsatisfactory smears for sex chromatin determination. The other possibility is that a small Y chromosome may fail to fluoresce. Borgaonkar and Hollander⁵ found a male with a small Y chromosome which did not fluoresce. We have recently had two cases where no sex chromatin body was found in 200 amniotic fluid cells and karyotypes of the fibroblasts cultured from one amniotic fluid revealed a 46,XX chromosomal constitution. No karyotypes were done on the other fluid, but both fetuses were found to be phenotypic females (our unpublished results).

Based on these observations, we feel that extreme caution should be used in prenatal sex determination by examination

for fluorescent Y and sex chromatin body. While the finding of a fluorescent body does indicate the presence of a Y chromosome and the presence of a sex chromatin body indicates the presence of an inactivated X chromosome, the failure to detect these bodies does not necessarily indicate the absence of the corresponding chromosome. It seems that until further experience with these techniques is obtained, karyotype analysis of cultured fibroblasts from amniotic fluid cells is the only accurate means of prenatal sex chromosome determination.

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Persistent Alteration of Turnover of Brain Noradrenaline in the Offspring of Rats subjected to Stress during Pregnancy

VARIOUS hypotheses have suggested that environmental stress during development is an aetiological factor in mental illnesses. Such hypotheses have included both biological and psycho-analytical concepts, but they have not explained convincingly any possible mechanism of action of stress. The developing organism has often been shown to be sensitive to various hormonal influences, and steroid and thyroid hormones or stress, given at a critical time during the neonatal period, seem to affect permanently the behaviour and metabolism of growing animals¹⁻³.

There have been several reports that stress or injections of adrenaline given to pregnant rats can have a persistent, although variable, effect on the behaviour of their progeny⁴⁻¹⁰. Although these changes in the offspring are probably mediated by some biological mechanism, they cannot be explained by any known persistent neurobiological changes. We have therefore made a neurochemical examination of the progeny of stressed pregnant rats, which could be a preliminary step in the construction of an animal model for human mental illnesses. We found that when stress was applied during pregnancy there

Table 1 Y Chromosomes, Sex Chromatin and Phenotypic Sex of Twelve Abortuses

Case No.	1	2	3	4	5	6	7	8	9	10	11	12
Y fluorescence	+	—	—	—	—	+	+	—	—	—	—	+
Sex chromatin	ND	ND	ND	ND	NS	NS	—	NS	+	NS	+	—
Phenotypic sex	M	F	F	F	M	M	M	F	F	F	F	M

ND, Not done; NS, not satisfactory; M, male; F, female; +, positive; —, negative.

was a long lasting effect on the metabolism of cerebral noradrenaline in the offspring.

Pregnant Sprague-Dawley rats were stressed from the fifteenth to nineteenth day of pregnancy with daily foot-shocks (BRS Foringer shock generator scrambler No. 001, 0.040 W for 5 min each day). When, in preliminary experiments, the shocking was continued beyond the nineteenth day of pregnancy, stressed mothers often ate their young. During the daily 5 min period of stress two pregnant rats were kept in the same cage. They frequently displayed either overt fighting behaviour or assumed a stereotyped fighting posture, rearing on their hind legs during the session. Control mothers were kept in similar cages equipped with a metal grid base for an equal time without the shocks. On the twentieth day of pregnancy, control and experimental rats were placed in individual cages until the birth of their offspring. Litters of eight to twelve rats each, of control and experimental mothers, were born on the same day after an average pregnancy of 21–22 days. After delivery the cages were not opened nor were the newborn rats handled before the age of 10 days. They were separated from their mothers at the age of 21–23 days.

of the fraction was measured by liquid scintillation spectrometry¹⁴. Corrections were made for the recovery of ³H-noradrenaline and endogenous noradrenaline from the columns.

The rate of decrease of the specific radioactivity of noradrenaline in telencephalon–diencephalon was found to be greater in the male offspring of the stressed rats, suggesting a faster turnover of brain noradrenaline in these animals. The half life of radioactive noradrenaline as estimated from the 1 and 3 h points was approximately 3.4 h in the control group but only 2.5 h in the experimental group (Table 1). In the second experiment, the increased rate of disappearance of radioactive noradrenaline in telencephalon–diencephalon was confirmed (Table 1). A smaller change, which failed to attain statistical significance, was also found in the mesencephalon–brain stem. Because the amounts of ³H-noradrenaline found in brains of both groups of rats were not different 1 h after intracisternal injection, it is unlikely that prenatal stress led to a decrease in the uptake of labelled amine. The concentrations of endogenous telencephalic–diencephalic noradrenaline were not significantly changed in either experiment, but in the second experiment the concentration was significantly increased in the brain stem (23%) (Table 1). Although this

Table 1 Effect of Prenatal Stress on the Turnover of Noradrenaline in the Rat Brain

		Concentration of noradrenaline (ng/g)	Specific activity (d.p.m./ng of ³ H-noradrenaline)	
			1 h	3 h
First experiment Telencephalon– diencephalon–anterior mesencephalon	Control	492 ± 29 (14) (100%)	1,344 ± 50 (7) (100%)	893 ± 63 (7) (100%)
	Experimental	480 ± 27 (14) (97%)	1,261 ± 76 (7) (94%)	689 ± 64 (7) (77%)*
Second experiment Telencephalon– diencephalon	Control	257 ± 28 (7) (100%)	—	836 ± 70 (7) (100%)
	Experimental	289 ± 11 (7) (112%)	—	566 ± 52 (8) (68%)†
Brain stem– mesencephalon	Control	510 ± 40 (6) (100%)	—	1,243 ± 120 (7) (100%)
	Experimental	627 ± 23 (7) (123%)*	—	1,001 ± 74 (8) (80%)

Data are mean ± s.e.m. (N) (% control).

* $P < 0.05$.

† $P < 0.01$.

The offspring of the stressed rats grew at the normal rate, and they had no gross neurological or behavioural abnormalities. At the age of 40–45 days the turnover of brain noradrenaline of the males was estimated by measuring the rate of disappearance of intracisternally injected radioactive noradrenaline. Under light ether anaesthesia, 5 μ Ci of 7-L-³H-noradrenaline (2.2 μ Ci/mmol, Amersham/Searle) was injected in 25 μ l. of physiological medium¹¹. After 1 or 3 h rats were decapitated, and brains and spinal cords were removed and dissected; the cerebellum was discarded. In each experiment, the control and experimental animals were from litters born on the same day, and the control and experimental animals were killed alternately. In the first of two experiments, the brain was divided by transection at the rostral limit of the pons, and the anterior part (telencephalon–diencephalon–upper mesencephalon) was studied. In the second experiment a transection was made just above the corpora quadrigemina and at the caudal limit of the medulla, and two regions were produced: the first region included the telencephalon and diencephalon, and the second region included the mesencephalon and brain stem. The separate brain regions were weighed while wet, and extracted in 12.0 ml. of 0.4 N perchloric acid in a motor-driven glass Potter–Elvehjem homogenizer and the homogenates were centrifuged at 10,000g for 20 min. Noradrenaline was isolated from the supernatants by adsorption on alumina chromatography columns¹². Endogenous noradrenaline in an acid eluate of the alumina columns was measured by a fluorometric method¹³, and the radioactivity

change in the concentration of noradrenaline in the brain stem needs further confirmation, it lends support to the conclusion that the turnover of this compound in the brain was increased after prenatal stress.

This increased turnover in the brains of 40–45 day old male rats after stress to their mothers during pregnancy could be a consequence of prenatal or postnatal effects¹⁰. For example, the rearing habits of the stressed mothers may have been altered. By using a cross-fostering technique, or allowing normal foster mothers to raise the offspring of both the control and stressed mothers it might be possible to resolve this question. But in our previous experiments both stressful neonatal handling (5 min each day) and intraperitoneal injections of adrenaline (10–20 μ g in oil, Parke Davis) of the newborn rats from the second to the tenth postnatal day had no effect on the turnover of noradrenaline. It seems more probable, therefore, that the effects we have noted were brought about during the development of the foetus. The effects of maternal stress could be mediated through a release of adrenal medullary catecholamine or corticosteroid hormones, both of which can cross the placenta and possibly enter the central nervous system *in utero*¹⁵. The effects of injections of adrenaline and hydrocortisone during the same gestational period on the turnover of noradrenaline in the progeny are now being investigated.

The increased turnover in the brain does not necessarily reflect changes in the resting metabolic rate of this putative neurotransmitter. The noradrenaline in the brain is quite

sensitive to various stresses¹⁶, and the offspring of stressed mothers could be merely more responsive to the stress of the intracisternal injection procedure. In this case, the change in metabolism of noradrenaline produced by prenatal stress would not be an isolated specific alteration of metabolism. Extensive (neuro)chemical and (neuro)histochemical screening will therefore be necessary to determine the entire metabolic sequences of prenatal stress.

The relevance of animal experiments to human studies can always be challenged. This is especially true in this case, for the time course of the development of rat brain is markedly different from that of man¹⁷. Rats are born relatively immature, and the greatest increase in the weight of developing rat brain occurs under considerable environmental stress after the birth, while in man this critical period of development occurs in the more stable and isolated conditions of the uterus. Therefore, the effects of stress even during the corresponding period of ontogeny may be quite different in man and in rat. Some other animals, for example the guinea-pig, could perhaps better serve as a model for human studies.

In spite of the difficulties of comparing different species, it is possible that intra-uterine experiences in man may also induce changes in brain metabolism, some of which might be sustained after birth and might alter behaviour^{15,18}. It is, however, too early to speculate about the theoretical implications of our observations for the aetiology of human mental illnesses. Nevertheless, it is interesting that in some recent epidemiological studies an abnormally high incidence of complications during pregnancy and delivery has been found among the schizophrenics^{19,20}.

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Reduction or Disappearance of Visual After Effect of Movement in the Absence of Patterned Surround

THE apparent movement of stationary patterns in the opposite direction to their immediately preceding real movement is among the earliest reported and most extensively investigated visual after effects¹⁻³. We have shown that the visual movement after effect is reduced or eliminated in the absence of a patterned surround, and that the presence of a surround is a stimulus condition necessary for its occurrence. Surround variables must, in future, be taken into account in explaining the movement after effect phenomenon.

The moving or stationary target was a pattern of vertical light and dark bars behind a 6 cm diameter circular aperture subtending a visual angle of 3° at the observation distance of 152 cm. The bars were 1.9 cm wide (0.75°) and were illuminated from behind. During the stimulation period of 90 s the bars moved from left to right at 2.25° s⁻¹ and during the test period, which followed immediately, the bars were stationary. Depending on ambient light the luminance of the light bars was between 7.30 and 11.92 cd m⁻², insufficient alone to cause variations in the movement after effect⁴. Without room lights, only the light bars of the moving target were visible. A central fixation point was marked on a clear plastic cover over the aperture. After 90 s of fixation during movement of the bars, the observer recorded apparent opposite motion of the stationary bars by manually moving a carriage to track apparent movement and to record it on a moving chart. Tracking the movement after effect by hand motion, also used by Honig⁵, was preferred to the more common methods of estimating its duration and "cancelling" it by a counter-movement of the target. The validity of this method has been established by requiring observers to track slow real movement.

The first experiment followed the chance observation that after fixation of the target in darkness there was little or no movement after effect. Four conditions were arranged to investigate this reduction and to establish whether it is due to the surround being dark and without pattern during either or both (stimulation and test) phases. A circular, 66 cm diameter surround (24°) with vertical, 1.9 cm (0.75°) wide black and white bars was mounted concentrically around the target and illuminated indirectly by incandescent sources so that the luminance of the light bars was 11.92 cd m⁻². The target of nearly constant luminance was clearly visible throughout. Referring in order to the stimulation and test phases and to the patterned and dark surrounds as P and D respectively, the four conditions were PP, PD, DP and DD. Twenty-four people observed in all conditions, each in one of the twenty-four possible orders. After three familiarization trials, observers were given a 10 min break and then the four test trials with 3 min breaks between. Mean movement after effects are shown as cumulative apparent distance moved by the bars over 10 s in Fig. 1A. The movement after effect for DD was markedly less than that for PP with those for DP and PD intermediate. One of the twenty-four observers failed to indicate a movement after effect in PP, two in DP, seven in PD and eleven in DD.

Differences in movement after effect due to absence of surround in inducing and test phases were both significant (inducing phase, F , 1.23 = 17.54, P 0.001; test phase, F , 1.23 = 37.81, P 0.001). Moreover, the movement after effect for PD was half that of DP, suggesting that the surround pattern is more significant in the test than in the inducing phase.

It is unclear from these findings whether the diminished movement after effect is due to sheer absence of light or lack of visible pattern in the target surround. In a second experiment twenty-four observers recorded the movement after effect as the luminance of the patterned surround was reduced from 11.17 to 0.11 cd m⁻². This large reduction in surround luminance reduced the movement after effect only marginally. This suggested that absence of pattern in the surround rather than

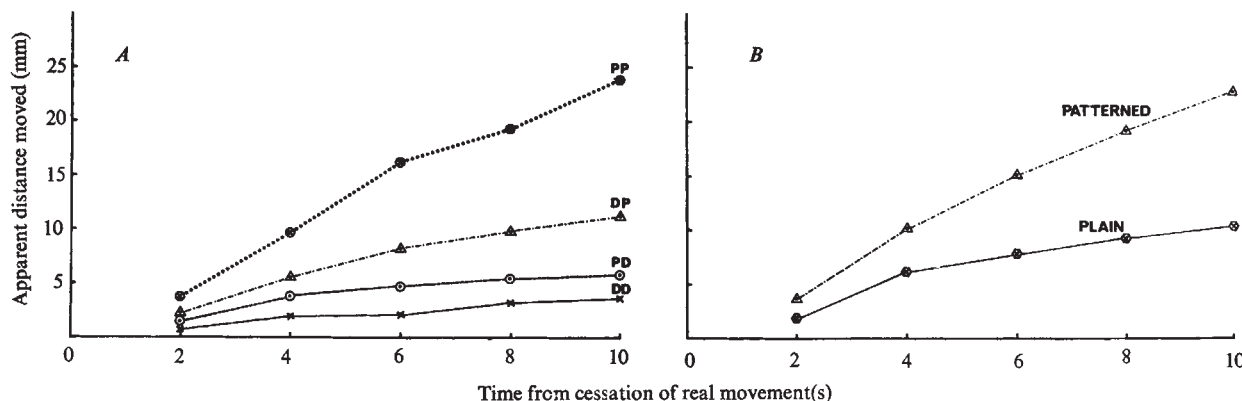


Fig. 1 (A) Cumulative apparent distance moved by stationary bars during 10 s in experiment 1. P, Patterned surround and, D, dark surround during stimulation and test phases in that order. (B) Cumulative apparent distance moved by stationary bars during 10 s in experiment 3 with patterned and plain surround.

absence of light was the critical determinant. A third experiment was therefore conducted, using the patterned surround of the two earlier experiments and a plain white surround of the same size, shape and material. The luminance of the light area of both surrounds was 0.11 cd m^{-2} . Twelve subjects observed with both surrounds with the orders and presentation counter-balanced (Fig. 1B). With the plain surround the movement after effect was much less than with the patterned surround ($F, 1, 11 = 7.30, P 0.05$). Since the total light reflected from the plain white surround was greater than from the barred pattern it can be concluded that the reduced movement after effect with a uniform dark or light surround was a consequence chiefly of the absence of visible pattern.

In another experiment with twelve subjects, we found that there was no significant difference in the movement after effect produced when the surround pattern consisted of regularly arranged dots, concentric circles, a grid pattern or vertical bars. Thus while a patterned surround is a critical determinant of the movement after effect, the particular pattern is not. Variables such as orientation of surround contours, however, require systematic investigation.

In experiments with rotated sectorized disks we have obtained a less striking reduction in the movement after effect than that occurring with linear moving targets in the absence of surround pattern.

Visual movement can be classified conveniently as relative or egocentric movement. In the case of relative movement objects are observed to move relative to other stationary or moving objects, but with egocentric movement they are observed as moving relative to the observer only, for the visual field is dark or featureless. The findings reported here indicate that the movement after effect is essentially a relative movement phenomenon, and it can be assumed that the neural processes correlated with the after effect derive from stimulation by moving and stationary contours. This finding is supported by Wohlge-muth's observation³ that the after effect occurs only feebly after movement of the total visual field. If, as suggested^{6,7}, the neural processes associated with the movement after effect are indicative of those involved in real movement, it is reasonable to suppose that the latter is encoded in relative terms.

It has occurred to us that the presence of a patterned surround may also be a significant determinant of other after effects such as those associated with tilted contours⁸ and orientation specific after images obtained with stationary and moving patterns⁹⁻¹¹.

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Primary Reaction of Phytochrome

MANY aspects of morphogenesis in higher plants are regulated by phytochrome^{1,2}, but the mechanism of its action is still a subject for controversy³. By "primary reaction of phytochrome" we mean the first reaction in which P_{FR} , the physiologically active species of the phytochrome system which absorbs far red light, becomes involved after its formation ($P_{FR} + X \rightarrow P_{FR}X$). This reaction has not yet been identified. The chief problem is whether or not the reaction partner or reaction site X is the same in all photoresponses mediated by phytochrome. For some reason it is generally believed that the primary reaction of P_{FR} is the same in all cells³. With the same confidence, however, one can maintain that, even in the primary reaction, P_{FR} acts differently in different cells or in different compartments of a particular cell².

Recent findings support the latter alternative. There are at least two sets of data which relate to this problem. First, while the results obtained by Hartmann^{4,5} with lettuce seedlings (hypocotyl lengthening) and by Oelze-Karow *et al.*⁶ with mustard seedlings (threshold regulation of lipoxygenase synthesis) probably require a cooperative model for the primary reaction of P_{FR} , Lange *et al.*⁷ have shown for phytochrome-mediated anthocyanin synthesis in the mustard seedling that there is no need to postulate any cooperative effect between P_{FR} molecules. Second, while for phytochrome-mediated photomodulation of ascorbic acid synthesis in the mustard seedling, the control by P_{FR} is independent of RNA synthesis⁸, phytochrome-mediated anthocyanin synthesis^{7,9} and a number of other responses² probably involve regulation of gene activity.

Here we compare some details of phytochrome-mediated ascorbic acid and anthocyanin synthesis in the same system (mustard seedling, *Sinapis alba* L.). Our data are relevant to

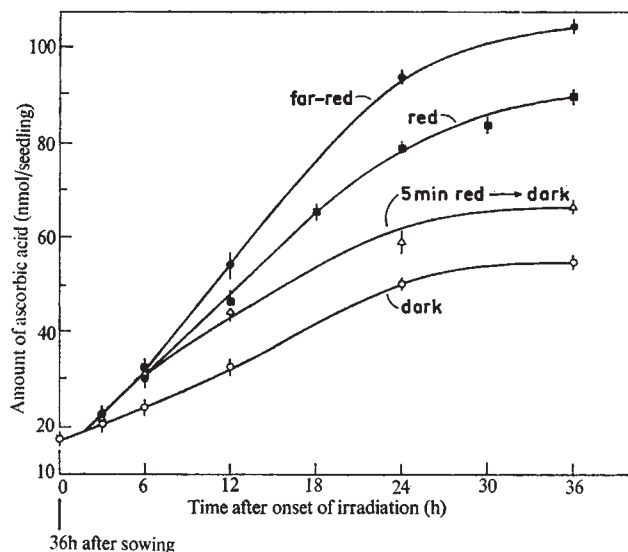


Fig. 1 Time course of ascorbic acid accumulation under continuous far red light, continuous red light and after one brief irradiation (5 min) with red light given at time zero (that is, 36 h after sowing).

the problem of whether or not the primary reaction of P_{FR} is the same in both responses.

Seed of white seeded mustard was bought from a commercial grower (Schoell, Stuttgart). Selection of seeds and treatment of seedlings followed the standard techniques for photomorphogenic research with mustard seedlings developed in our laboratory¹⁰. The seedlings were grown at $25.0 \pm 0.2^\circ \text{C}$ in the dark, and light treatment was started 36 h after sowing—time zero in the figures.

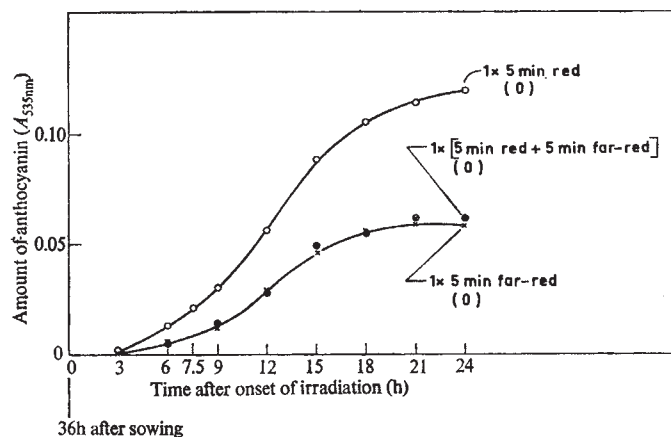


Fig. 2 Time courses of anthocyanin accumulation after one or two brief irradiations (5 min each) with red light, far red light, or red followed by far red light. The irradiations were performed at time zero (that is, 36 h after sowing).

For irradiation, the standard red¹¹ and far red¹⁰ sources were used at an irradiance of $67.5 \mu\text{W cm}^{-2} \pm 10\%$ (red) and $350 \mu\text{W cm}^{-2} \pm 10\%$ (far red). The temperature during irradiation was $25.0 \pm 0.4^\circ \text{C}$. The methods of extraction and determination of ascorbic acid and anthocyanin have been described elsewhere^{8,9,12}. The values in the figures are means of between eight and sixteen parallel but independent experiments. Standard errors were about 2–5% (or indicated by the vertical bars).

Five minutes of red light (which establishes a phytochrome photoequilibrium of about $[P_{FR}]/[P_{TOT}] = 0.8$) applied to a dark grown seedling at time zero will transiently increase the

rate of ascorbic acid accumulation (Fig. 1). Because the effect of 5 min of red light can be fully reversed by an immediate irradiation with 5 min of far red light¹², we conclude that P_{FR} in the ground state (non-excited) can effectively control the rate of ascorbic acid accumulation. Similarly, 5 min of red light applied to a dark grown seedling at time zero causes anthocyanin synthesis (Fig. 2), and as the effect of 5 min of red light can be fully reversed by immediately following it with 5 min of far red light (Fig. 2), we conclude that P_{FR} in the ground state can cause anthocyanin synthesis.

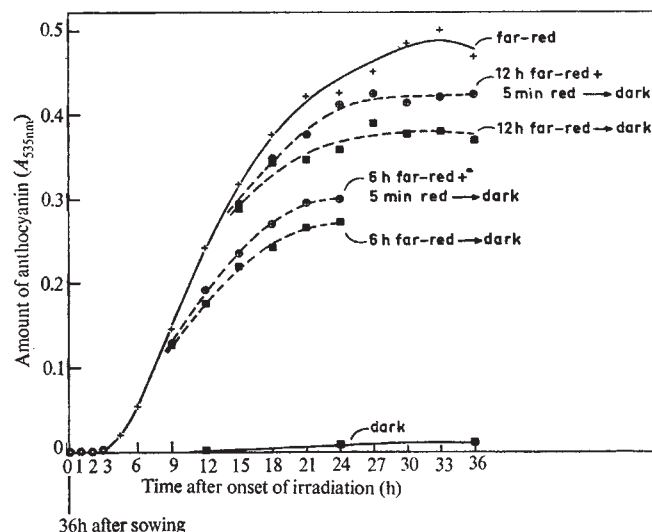


Fig. 3 Time courses of anthocyanin accumulation under continuous far red light and in experiments where the standard far red light was turned off 6 or 12 h after the onset of irradiation. The samples were either placed in the dark immediately or received 5 min red light before the transfer to darkness.

Continuous far red light (which will establish a phytochrome photoequilibrium of about $[P_{FR}]/[P_{TOT}] = 0.03$) exerts a strong effect on anthocyanin synthesis (Fig. 3). The rate of anthocyanin synthesis in the linear part of the curve is a log function of the irradiance in these circumstances⁷. This dependency on irradiance in conditions of continuous irradiation is a characteristic of the so-called "high intensity reactions". According to

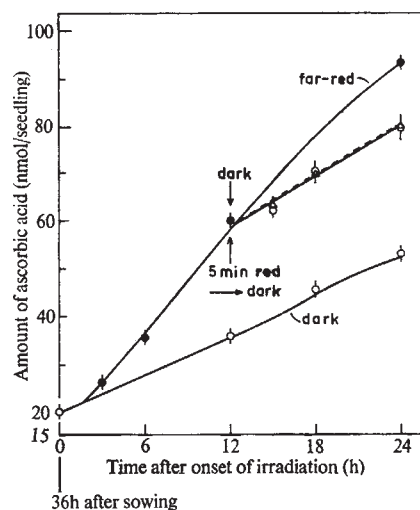


Fig. 4 Time course of ascorbic acid accumulation under continuous far red light and in an experiment where the standard far red light was turned off 12 h after the onset of irradiation. The samples were either placed in the dark immediately or they received red light for 5 min before the transfer to darkness.

Hartmann^{4,5}, this effect can be attributed to the phytochrome system as well. And indeed, we have found no data which could not be reconciled with the hypothesis advanced and supported by Hartmann¹³ that the morphogenic effects of continuous light of wavelength greater than 550 nm can be attributed to phytochrome⁷. Fig. 3 shows that even after prolonged treatments with continuous far red light, the anthocyanin producing system of the mustard seedling responds to changes of the P_{FR} level (exerted by 5 min of red light after the termination of far red light) as would be expected: an increase of P_{FR} in the ground state from about 3% P_{FR} (photoequilibrium characteristic for red light) leads to an increase in the rate of anthocyanin synthesis. Corresponding experiments for ascorbic acid accumulation lead to a different result (Fig. 4). The ascorbic acid synthesizing system of the mustard seedling does not respond at all to changes in the level of P_{FR} (exerted by 5 min of red light after the termination of far red light). The programme: 12 h far red light plus 5 min red light→dark leads to the same results as the programme: 12 h far red light→dark. Obviously the system can no longer respond to P_{FR} in the ground state while the sensitivity towards continuous far red (that is, phytochrome in some excited state) is not significantly decreased.

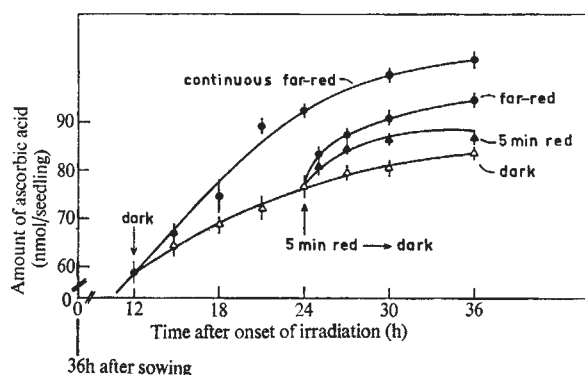


Fig. 5 Time course of ascorbic acid accumulation under the programme: 36 h dark (time zero)—12 h far red light—12 h dark—5 min red light→dark. The corresponding control kinetics are given as well: 12 h far red light→dark (dark); 12 h far red light—12 h dark→far red light (far red); continuous far red light.

The question arises as to whether or not the sensitivity of the system towards P_{FR} in the ground state can be restored by a dark period. This is indeed the case. Fig. 5 shows that after a dark period of 12 h (following 12 h of continuous far red light) the ascorbic acid synthesizing system of the mustard seedling again responds to P_{FR} even in the ground state (5 min red→dark).

The anthocyanin synthesizing system and the ascorbic acid accumulating system of the mustard seedling clearly respond differently to a treatment with continuous far red light. For anthocyanin synthesis, sensitivity towards P_{FR} (in the ground state) is preserved, but for ascorbic acid accumulation it is lost after 12 h of continuous far red light. In both cases, however, sensitivity towards continuous far red (P_{FR} in some excited state) did not change significantly during the experimental period (compare Figs. 3 and 4, continuous far red). Under continuous far red light both responses (anthocyanin synthesis as well as ascorbic acid accumulation) follow virtually steady state kinetics during the experimental period (between 6 and 18 h after the onset of irradiation). Any model which claims to describe the universal reaction, $P_{FR} + X \rightarrow P_{FR}X$ (ref. 3), must explain these facts. We feel that such a universal model is difficult to formulate if it is at all feasible. For this reason we prefer the hypothesis (Fig. 6) that X is not the same for every plant photoresponse. We frankly admit that a detailed model

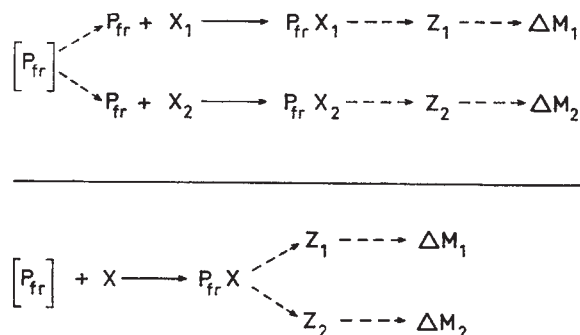


Fig. 6 Two alternative models of the primary reaction partner (or site) of P_{FR} (in the ground state). The lower model is not consistent with the facts reported in this paper. While other, more complicated, models might be conceived to explain the available experimental information, the upper model has the advantage of being simple as well as consistent with the facts. $[P_{FR}]$: P_{FR} -pool of the system; $X_{1,2}$: reaction partner(s) or site(s) of P_{FR} . Nothing is implied about the degree or type of difference between the different X s. $Z_{1,2}$: intermediate(s) between $P_{FR}X$ and the responses; ΔM : extent of P_{FR} mediated responses (in the present case, increase of rate of synthesis).

of the process $P_{FR} + X_i \rightarrow P_{FR}X_i$ is not available at the moment, neither for phytochrome-mediated ascorbic acid accumulation nor for phytochrome-mediated anthocyanin synthesis. We hope, however, that results like those presented here will open the way to a better understanding of the primary reactions of P_{FR} , for we have good reason to ask: "What is the primary reaction of P_{FR} ?" for every sort of photoresponse.

We propose the hypothesis that the reaction partner (or site) of P_{FR} (X) can be different in different cells or in different compartments of a particular cell and that for this reason we can expect a multiplicity of "primary reactions of P_{FR} ".

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Coupled Oscillatory Control Mechanism in a Planktonic System

THE mechanism by which soluble organic matter is released in lakes and the rate at which this occurs are of great importance relative to the development and rate of metabolism of populations of heterotrophic bacteria. One potentially large source

of soluble organic matter is extracellular release by phytoplankton which are actively photosynthesizing. It has been shown that rates of excretion of organic matter by algae are a function of the relative growth rate of the algae in culture^{1,2}. In conditions of constant light, there is a tendency for the net excretion rate to be more or less constant initially and then to approach zero after 3–5 h.

Glycolic acid, amino-acids, sugar alcohols, peptides and polysaccharides have been the principal substances detected in excretions of algal cultures^{3,4}. The simple substrates excreted are probably derived from small metabolic pools that are saturated very soon after exposure to light⁵. There is evidence that *Thalassiosira fluviatilis* excretes largely newly formed organic matter, although a small percentage of the carbon excreted may come from older cellular constituents⁴.

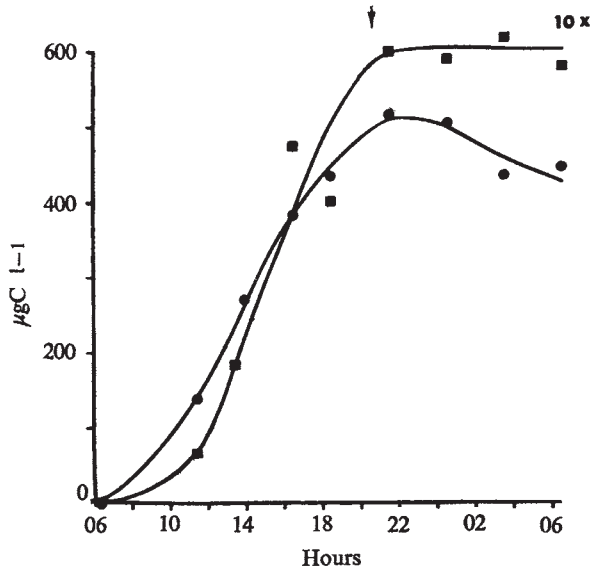


Fig. 1 Net photosynthetic carbon fixation and extracellular release by phytoplankton at 1 m in Frains Lake, Michigan, July 22, 1968. ●—●, Photosynthesis; ■—■, extracellular release. Arrow indicates sunset.

The kinetics of extracellular release, throughout a 24 h period, of soluble organic matter by phytoplankton *in situ* have not previously been examined in detail, although *in situ* experiments have been conducted during the mid-solar day³. Fig. 1 shows an example of cumulative net photosynthetic carbon fixation and extracellular release by phytoplankton at 1 m in Frains Lake, Michigan, on July 22, 1968. $\text{NaHC}^{14}\text{O}_3$ was used as a tracer. It is assumed that the extracellular organic matter is derived entirely from newly formed photosynthate. The amount released can thus be calculated directly knowing the specific activity of the inorganic pool available for photosynthesis. During the daylight period the rates of both processes fluctuate from zero to some high value and back to zero. The rates would fluctuate even if the extracellular matter was derived from older cellular material. The average hourly rates of net photosynthetic fixation and extracellular release are plotted in Fig. 2. In this case the excretion rate lags behind the photosynthetic rate, with the maxima differing by approximately 4 h.

If the enzyme systems for the bacterial community are not saturated initially for the substrate released extracellularly, the metabolic rate of the bacteria could be predicted to vary with the concentration of substrate. This rate would vary approximately as the assimilation rate of substrate according to the well known Michaelis-Menten enzyme kinetics. Some adjustment of the bacterial enzyme systems to increasing levels of substrate might also be expected, particularly at high levels. Thus the response of the bacteria would lag behind the release of

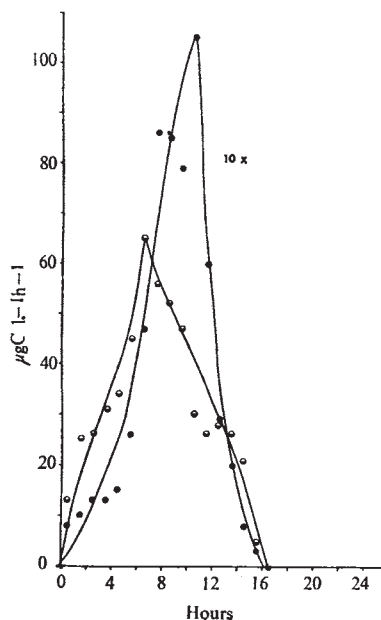


Fig. 2 The average hourly rate of net photosynthetic fixation and extracellular release by phytoplankton at 1 m in Frains Lake, Michigan, July 22, 1968. ●—●, Photosynthesis; ■—■, extracellular release.

substrate by the phytoplankton. The rate of assimilation would decrease again as the substrate was assimilated by the bacteria.

We carried out a simulation experiment to test for a fluctuation in the rate of assimilation by the bacteria of an extracellular soluble organic pool. Unmodified water from the lake surface was placed in a 5 gallon carboy which was then suspended at the surface of the lake. Glucose was added to the carboy hourly at a rate which simulated a known rate of extracellular carbon release by natural phytoplankton communities (Fig. 3) in a productive lake. The experiment was initiated at sunrise and was terminated the following sunrise. At hourly and later two hourly intervals the carboy was subsampled. Duplicate 300 ml. biochemical oxygen demand (BOD) bottles were filled with the subsample and inoculated with tracer quantities of glucose-C-14(U). The bottles were wrapped with

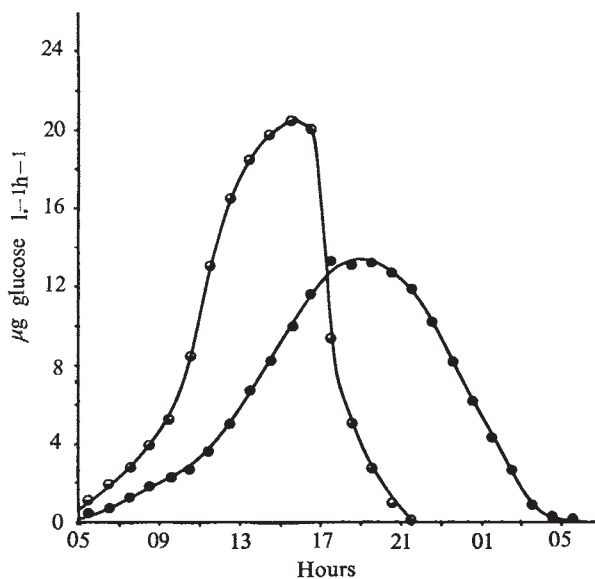


Fig. 3 Simulation experiment for phytoplankton extracellular release and bacterial assimilation. Wintergreen Lake, Michigan, July 10, 1970. ●—●, Extracellular release; ■—■, bacterial assimilation.

aluminium foil to ensure complete darkness and were incubated in the surface water for 34 min. After this time 20 ml. subsamples were injected into sealed absorption vessels containing 1 ml. of concentrated phosphoric acid. The pH of the injected sample was reduced to 1.0 so that further metabolism of glucose was stopped and all bicarbonate transferred to carbon dioxide. The carbon dioxide was allowed to diffuse into a vial containing a carbon dioxide absorbant. The radioactivity in the vial was subsequently determined by scintillation counting.

A second set of subsamples was filtered through 'HA Millipore' filters into a preservative to prevent further metabolism of glucose. The filtrate was acidified and degassed of inorganic carbon. This treated sample was then plated, dried and the radioactivity was counted. The radioactivity in the bacteria on the filter was also determined. In this way a carbon-14 radioactivity (mass) balance was obtained for each tracer assay. There were no discrepancies in the carbon-14 balance for any tracer assay.

The sum of the carbon dioxide and bacterial carbon-14 radioactivity produced per unit time was considered to be the gross or true assimilation rate of ^{14}C -glucose. Knowing the simulated extracellular release rate of glucose-C-12, the bacterial assimilation rate of glucose-C-12 was calculated for each tracer assay. The results (Fig. 3) demonstrate that a fluctuation in the extracellular rate of release of glucose is accompanied by a fluctuation in the bacterial assimilation rate of glucose. There is a phase difference in the maxima of the two rates of slightly more than 3 h. This experiment thus proves the general biological phenomenon. The initial concentration of glucose in the lake water was not known. It was assumed to be $1 \mu\text{g l}^{-1}$ for purposes of the calculation. It can be shown for this experiment, however, that the bacterial assimilation rate will oscillate even if the initial concentration of glucose was $100 \mu\text{g l}^{-1}$. The latter value is much greater than any known concentrations of glucose in lake water.

Any substrates released by phytoplankton which have a relative utilization rate similar to glucose will produce a similar oscillation in the assimilation rate. The period of the oscillations will be 24 h and the substrate will be almost completely utilized within 24 h. A substrate that is utilized more slowly than glucose will also produce a fluctuation in the bacterial assimilation rate. The phase difference between extracellular release and bacterial assimilation would tend to be much longer. The ultimate response of the bacteria could be much more complex than in the foregoing example.

It is possible, therefore, to view the total reaction sequence of light, phytoplankton, extracellular soluble organic matter, and bacteria, as a linearly coupled fluctuating control system. In this system light intensity is a forcing environmental variable and the phytoplankton-bacteria constitute a biological response system where the coupling between phytoplankton and bacteria is mediated by a fluctuating soluble organic pool. The intensity of each response will oscillate and will be somewhat out of phase with the preceding component of the system. This sequence constitutes a basic control mechanism for bacterial development.

Such a basic control mechanism provides a basis, in part, for analysing the population development of phytoplankton and bacteria in lakes and for discerning additional control mechanisms which operate to dictate what that development might be.

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Food Chain Model for DDT Kinetics in a Freshwater Marsh

EQUATIONS of first order kinetics have been suggested as potentially suitable models for the accumulation of DDT in living organisms¹⁻³. Although much has been written about the use of such models in analysing "tracer" studies⁴, and in studying the fate of radionuclides in ecosystems^{5,6}, little has been done to construct a model to represent field data for pesticides. Knowledge of the kinetics of pesticides in ecosystems is at present insufficient for completion of the model, but we believe that this will be possible when certain problems have been solved. A full account of the field study⁷ and a detailed report of the model are available⁸.

The data of Fig. 1 were obtained using DDT labelled with chlorine-36 on the phenyl ring. This DDT was applied in an inert granular carrier⁷ from a helicopter to an enclosed 4 acre marsh at the south-western edge of Lake Erie in July 1964, at a rate of 0.2 pounds of technical grade DDT/acre (220 g/ha). Our model assumes an exponentially decreasing rate of release of DDT from the granules into an average of about 12 inch (30 cm) of water, followed by transfer to suspended materials (chiefly phytoplankton) and various items of the biota according to equations of the form

$$\frac{dy_2(t)}{dt} = c_2 y_1(t) - \mu_2 y_2(t) \quad (y_1(0) = 0)$$

where $y_2(t)$ represents DDT concentration in a given "compartment", c_2 is a "transfer coefficient", μ_2 is a rate of loss and $y_1(t)$ is the concentration of the source of DDT taken in by the given compartment. The concentration of DDT in filtered water is assumed to follow the equation

$$\frac{dy}{dt} = \lambda e^{-kt} - \mu y$$

where λe^{-kt} represents release from granules, and μy losses from water due to all causes. Rate constants for this equation were obtained by a nonlinear least-squares fit to its solution, neglecting the two points represented by open circles in Fig. 1. A more realistic model would assume recycling of DDT, so that releases of DDT from various components might again be taken up from the water. But such a model does not seem feasible with the available data. Codistillation of DDT with water is potentially important for the determination of the fate of DDT in an aquatic system. Some laboratory experiments have shown losses through codistillation of more than 50% of the quantity introduced within 24 h (refs. 9, 10), but we have no relevant data.

Rate constants for twenty different species were obtained, five of which seemed to fit realistically into the food chains of Fig. 1. In many cases we assumed that individual species had two compartments, one representing long-term retention and the other (a "fast" compartment; shaded areas in Fig. 1) representing transient conditions when the concentration of DDT in the water was relatively high. We assumed that the fast compartment was essentially depleted of DDT in the first 14 days after application, and used the observations on and after the fourteenth day (usually four points: 14, 30, 60 and 90 days after application) to estimate rates of loss and

transfer for the long-term ("slow") compartment. Contents of the fast compartment were then assumed to constitute the difference between observations and a computed curve for the slow compartment. These differences were usually maximal either 24 or 72 h after application, so we arbitrarily assigned rates of loss which yielded corresponding maxima in a computed curve. Fast and slow compartments were added to constitute the total DDT burden of a given species. Because our parameter estimates for the fast compartment seem unreliable we suggest the use of autoradiography to study possible plant "compartments" in controlled experiments. It is quite possible that our "fast" plant compartment is, in part at least, associated with periphyton. We assume that the "slow" compartment constitutes DDT somehow bound to plant tissues, but note that the available evidence suggests DDT is not translocated by plants, so that a likely candidate for the slow compartment is the cuticle¹¹.

ment with inputs from a transient water surface concentration of DDT (probably a DDT-xylene-dust aggregation). Both carp and green sunfish had maximal concentrations of DDT of the order of 30 days after application, and so we assumed that they received most of their DDT through the food chain. An initial very rapid increase in concentrations in carp was assumed to be due to a fast compartment receiving DDT directly from the water. We have too few data to determine reliably the dominant mode of DDT uptake by fish in field conditions. Direct uptake from water has been demonstrated^{13,14}, and high concentrations may readily be obtained from food containing DDT^{15,16}. Our observations, and others¹⁷, suggest that maximum concentrations are associated with the food chain, but controlled experiments are needed in more or less natural conditions. A promising approach seems to be the use of differentially labelled DDT, with one "tag" in water and the other in food items—DDT labelled

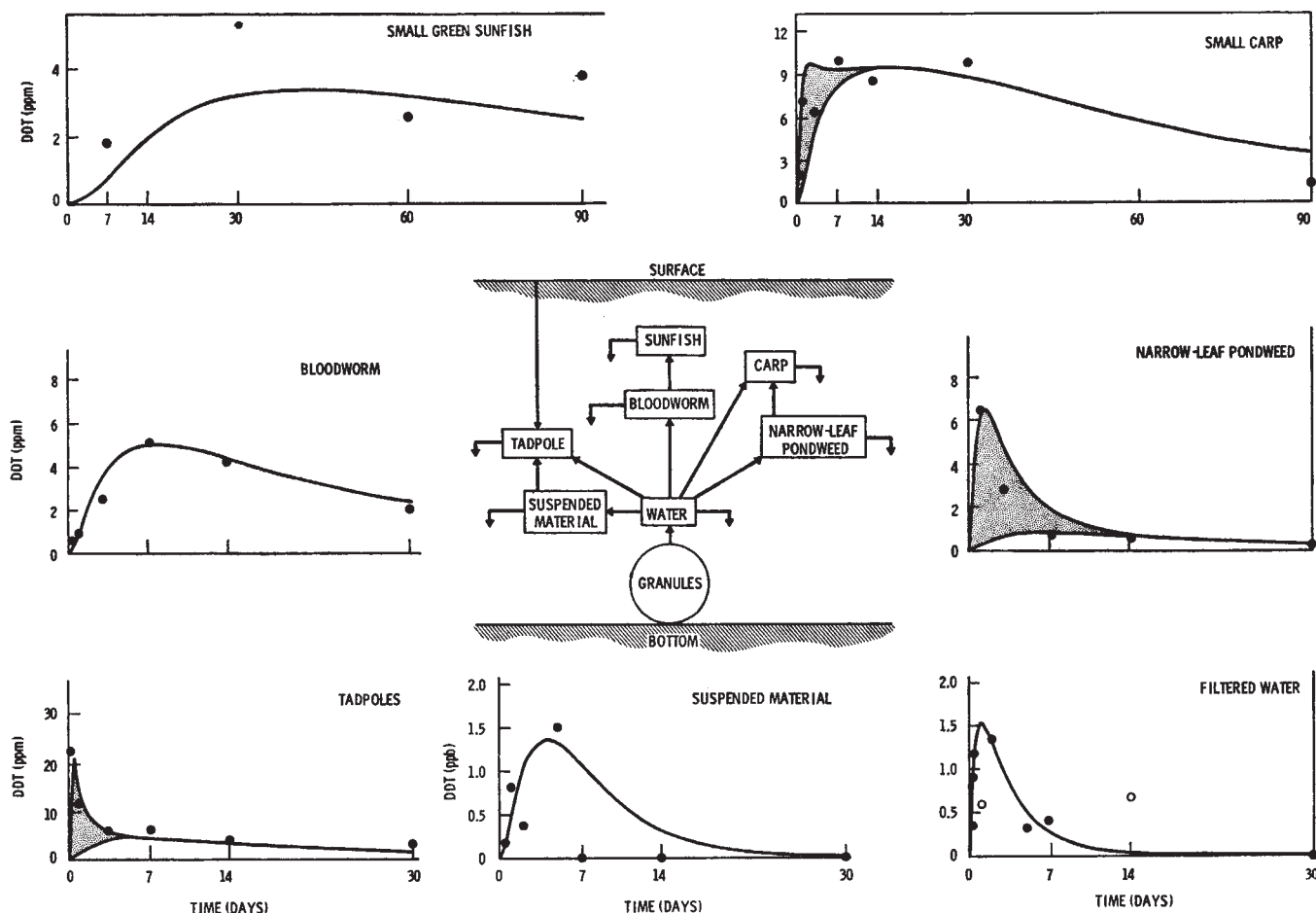


Fig. 1 Food chain model for selected items of the biota of a freshwater marsh. Plotted points represent observed concentrations of DDT at various times after application. Solid curves represent concentrations computed from the model equations. Shaded areas (carp, tadpole and narrow-leaved pondweed) represent the contribution of a "fast" compartment.

One of the puzzling outcomes of this study is that concentrations of DDT in suspended matter (largely phytoplankton) decrease below detection limits 7 days after application. A few parts per billion of DDT can appreciably reduce photosynthesis by marine phytoplankton¹², but we have no basis on which to argue for or against a similar effect in our study.

Although all ten of the plant species modelled required two compartments, it seemed that most of the six invertebrates could be represented by single-compartment models. Tadpoles contained very high DDT concentrations (23 p.p.m.) within a few hours of application, so we have assumed a fast compart-

ment with carbon-14 or chlorine-36 is available. We should point out that what we call "DDT" undoubtedly includes various degradation and metabolic products (especially in the fauna). Because the phenyl ring of the chlorine atoms remains attached to all known DDT metabolites⁷, we are confident that our data represent the fate of DDT and its metabolic and degradation products, but an unrealistically large effort would be required to establish relative proportions of the various constituents in time.

Our estimates of transfer coefficients and rates of loss for the long term compartments are summarized in Table 1. We

would like to emphasize that the use of concentration factors depends on the maintenance of a constant level of DDT in the water. Our two compartments are effectively assumed to be of equal mass (because we simply add concentrations in the two compartments to obtain overall concentration in a given species). In equilibrium conditions, concentration (dry weight)

in a given organism (y_i) is then

$$y_i = y_w \left[\frac{c_1}{\gamma_1} + \frac{c_2}{\gamma_2} \right] \equiv [y_w] [\text{concentration factor}]$$

where y_w is the concentration in water, and c_1 and γ_1 are respectively transfer coefficients and rates of loss. The usual interpretation of a concentration factor as the ratio of concentration in an organism to that in water may thus often be misleading when applied to DDT, and reliable measurements of concentrations in water are of crucial importance. In the case of a single surface application of DDT to an aquatic system, an approximation to a dynamic model may be helpful for planning purposes. If concentration in one compartment of an organism is represented by y , then

$$y = \frac{c\lambda}{k} e^{-\gamma t}$$

where c is the transfer coefficient, γ the rate of loss (Table 1), and decrease of DDT in water follows the equation

$$y_w = \lambda e^{-kt}$$

Further details and some crude estimates of λ and k are given in ref. 8. Suspended matter cannot be neglected, so that three separate measurements of water concentrations ought to be considered (a bulk water sample, suspended material and filtrate).

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Table 1 Parameters of DDT Uptake and Retention by Marsh Biota

Species	Long term compartment *				Conversion factor —dry to wet weight
	Transfer co-efficient	Rate of loss ($\times 10^4$)	s.e. ($\times 10^4$)	Concentration factor †	
Plants					
Sago pondweed (<i>Potamogeton pectinatus</i>)	9.1	1.8	1	53,000 (0.98)	0.12
Curly leaf pondweed (<i>P. crispus</i>)	7.3	0.9	5	84,000 (0.98)	0.17
Narrow-leaf pondweed (<i>P. foliosus</i>)	7.5	25.0	—	7,100 (0.42)	0.11
Water milfoil (<i>Myriophyllum</i> sp.)	8.0	7.8	5	11,000 (0.91)	0.17
Bur reed (<i>Sparganium eurycarpum</i>)	3.5	5.7	2	8,900 (0.68)	0.07
Soft stem bulrush (<i>Scirpus validus</i>)	13.0	39.0	14	5,500 (0.60)	0.09
Duckweed (<i>Lemna minor</i>)	45.0	55.0	35	11,000 (0.72)	0.11
Coontail (<i>Ceratophyllum demersum</i>)	13.0	15.0	4	15,000 (0.61)	0.13
Cladophora (<i>Cladophora</i> sp.)	130.0	59.0	18	83,000 (0.24)	0.26
Bladderwort (<i>Utricularia vulgaris</i>)	14.0	12.0	0.5	22,000 (0.56)	0.10
Invertebrates					
Red leech (<i>Erpobdella punctata</i>)	77.0	16.0	—	47,000 (1.00)	0.16
Crayfish (<i>Orconectes immunitis</i>)	27.0	13.0	2	22,000 (0.93)	0.23
Dragonfly naiad (<i>Anisoptera</i>)	—	7.6	7	—	0.18
Backswimmer (<i>Notonectidae</i>)	30.0	28.0	4	10,000 (1.00)	0.24
Bloodworm (<i>Tendipes</i> sp.)	42.0	17.0	2	25,000 (1.00)	0.19
Ramshorn snail (<i>Planorbidae</i>)	13.0	32.0	20	4,700 (0.89)	0.58
Vertebrates					
Carp (<i>Cyprinus carpio</i>)	—	7.8	3	—	0.23
Green sunfish (<i>Lepomis cyanellus</i>)	—	5.9	2	—	

* Coefficients expressed on a per hour basis.

† Figures in parentheses give proportion due to long term compartment.

BOOK REVIEWS

Paradigms and Policy

Science Studies: Research in the Social and Historical Dimensions of Science and Technology. Edited by Roy MacLeod and David Edge. Quarterly. Volume 1, Number 1. Pp. 115. (Macmillan: London, January 1971.) £5.00 annually; £1.50 each copy.

"SCIENCE," one might feebly quip, "is too serious a matter to be left to the scientists." The time has come for historians, philosophers, sociologists, economists, and the like to tell us what we are really doing and what we ought to do. The past decade is notable for the ascent into academic consciousness of a new interdisciplinary discipline—the study of science itself. *Science Studies* is to be a vehicle for the publication of scholarly articles on this subject. It is very welcome: may it flourish.

As every sociologist of science now knows, a new journal signifies the budding of a new invisible college. With active groups already established in several universities, we may anticipate rapid growth for the darling child. It is perhaps worth enquiring into its parentage and prospects.

On the distaff side, we observe the ancient scholarly house of History and Philosophy of Science, whose lineage may be traced back at least to Francis Bacon and Bishop Sprat. Here already there is no lack of journals, conferences, paradigms and pundits—not to mention perennial controversies of the kind exemplified by David Bloor's Essay Review of the Kuhn-Popper tourney. No great disrespect is intended if I suggest that this sort of thing is certainly most interesting to those interested in this sort of thing, but that it has not quite lived up to its high ideal of throwing floods of light on the scientific enterprise.

The other proud parent is sociology, whose cloud of pedantic polysyllables has begun to envelop the scientific community. Shrewdly handled, the professional headlamps of social enquiry can illuminate many features of the scholarly life—for example, the "norms" of scientific behaviour, or the means by which controversies are controlled. Unfortunately, conceptual schemes originally devised for the analysis of the role structure of the Navajo Indians are not quite adequate when applied to a tribe of astrophysicists and molecular biologists. Nor does a quick gallop through the conventional literature, as in R. G. A.

Dolby's "Sociology of Knowledge in Natural Science", leave one with a 'Eureka' feeling (sorry—having experienced an *Aha-erlebnis*). J.-J. Salomon on "The *Internationale* of Science" is pleasant, but also unprofound, at the level of high-brow journalism.

Or is the brat a bastard born of practical expediency? Science costs money, and has become a decisive factor in the economic and political domain. Problems of "scientific choice", which used to be settled discreetly in the basement of the *Athenaeum*, now erupt into the public eye. Expertise on cost benefit analysis, technological forecasting, international administration and other useful arts has become essential equipment for the worldly scientist and his political allies. This is the "Real World of Science Policy" in which K. Kreilkamp sets the *Hindsight* project, and the screen on which D. J. de S. Price projects his logistic extrapolations of the exponential growth curve, with typical numeracy. It is also the arena of moral concern and political ideologies described so vividly and appositely by P. G. Werskey in his historical account of "British Scientists and 'Outsider' Politics, 1931–1945".

The genesis of this journal therefore presents splendid opportunities for hybrid intellectual vigour. Let us cross Kuhn with Price and put numbers into the paradigms. Polanyi's idealism and Werskey's realism would protect Mertonian sociology from the fungus of academicism. A few Popperian genes might mask some of the lethal mutations of neo-Marxist social relevance—and so on. Above all, let us have more from active scientists—men and women who know from experience the joys and pitfalls of the search for a philosophy of nature. The editorial board is too heavily loaded with professional "Science Studies" scholars. What about Blackett and Medawar, Zuckerman and Weisskopf, Kothari and Salam, who have made science policy themselves and understand it quite as well as the academic analysts and commentators? "Science Studies," one might retort, "are too serious a matter to be left to the Students of Science."

My real fear is that the infant will be fattened on a pappy diet of "normal" philosophy, history and sociology, spiced with jolly little enumerations of the trees of citations in molecular biology. It should be brought up as a spartan, to digest hard lumps of logic and linguistics, developmental psychology and economic statistics, and to

strive manfully with such invincible opponents as scientific education, technological responsibility and national development. Many clichés of the conventional wisdom are waiting to be challenged, and many heretical concepts are demanding to be adopted. "Science" is a fit topic for the most diverse scholarly techniques, but the mere fact of its "importance" is not enough to justify flabby thought and implausible speculation. I hope, therefore, that the editors will take their duties seriously, and will give this journal a reputation for sharp-minded, diverse and original work on which we may lean with some confidence in the exercise of our communal profession.

JOHN ZIMAN

Costing Education

An Introduction to the Economics of Education. By Mark Blaug. Pp. xviii+363. (Allen Lane, The Penguin Press: London, November 1970.) £3.50.

It is interesting to ask whether or not education and science contribute to economic growth. Put like that, of course, the question is absurd, but it can be broken up into meaningful parts. To these much more specific questions, specific answers can be given, on the basis of extremely detailed studies of the content, structure and relevance of particular bits of education and science to actual situations. Of course, such answers will be based on a general structure of ideas. Natural scientists may suspect, but they probably do not usually know, that the structure of ideas in such disciplines as economics, history, and (say) textual criticism is different from—though allied to—the structure and organization of thoughts to which they are accustomed in their own disciplines, though, of course, it is incorrect to believe that there is a single general corpus of "science" that can be teased out from the individual disciplines of biology, physics and so on. Economics is not a science in the sense in which physics is; and those who assert that it is—the advocates of so-called "positive" economics—are as partisan as (though usually less frank than) those who take a more complex view of what their discipline actually is.

In education—itself a collection of highly disparate activities—economists can seek to analyse what has been spent, and what its consequences in the economic sense have been, and they can

also ask "ought" sorts of questions—what ought to be spent, and what would be the relevant range of criteria that might be brought to bear on such a question. Though the "is" questions are far from simple, the "ought" questions are extraordinarily complex, shot through with the social, political, ethical and cultural issues that necessarily dominate education and research.

One kind of "ought" answer has been manpower planning. It is fairly clear that, for the most part, manpower planning as a general strategy is a poor guide to educational planning. It dominated British university expansion in the 1960s. The results are fairly obviously undesirable. The physical sciences and technology have been disproportionately expanded, but this is not the direction of student choice. Research has been far too lavishly supported, but there are not enough jobs for the graduates. Britain combines one of the world's highest research efforts with one of the lowest rates of economic growth. All this is the result partly of the scientific lobby, and partly of a most inadequately studied assertion of a relationship between scientific research, trained scientists, and economic growth. Mark Blaug deals effectively with this sort of error in his new introductory textbook, though (to be honest) it is not an error that has been undealt with before.

What is more doubtful, however, is whether his chosen solution is any more help. Could we get a guide to the "correct" allocation of resources to education and science from the market, or from a paradigm of the market? Blaug is one of those economists (chiefly from the United States and its offshoots) who believe that economics is about markets. This so-called neo-classical view of economics is not shared by a great many people, and it seems to me to be curiously inapposite to the analysis of what are, for the most part, services that in most countries fall emphatically into the public sector of the economy. There are grounds, too, for believing that the answers derived from questions posed by such a mode of reasoning will be systematically misleading, for the market mode of reasoning necessarily asserts that there is (under competitive conditions) a set of prices, including the differential earnings of different grades of labour, which have some significance as guides to priorities. For those to whom price does not seem to play so large a role, and who are concerned with other questions, this will seem to lead to different answers to different questions.

There is, too, ground for further criticism of this volume. It seems to lack the intimate knowledge of the chosen field—education—that is desirable in a commentator. It is easy to

hand down prescriptions from a medical encyclopaedia if detailed clinical inspection of the patient is deemed unnecessary.

JOHN VAIZEY

Fossil Records

The Elements of Palaeontology. By Rhona M. Black. Pp. 339+4 plates. (Cambridge University: London, December 1970.) 4.50 boards; £2.25 paper.

THE reception of this book is bound to be controversial. Potential buyers are well advised to turn from the comments on the jacket to the words of the preface before assessing it. Here it clearly states that the book's objective is to introduce all the principal groups of invertebrate fossils and an outline of the palaeontology of the vertebrates and plants to students who require a knowledge of a wide field in palaeontology at an introductory level. The preface continues: "Some emphasis is given to biological aspects of palaeontology . . ." whereas the dust jacket misleadingly states: "Particular attention is given to the biological aspects of palaeontology." Similarly the dust jacket leads the reader to believe the book is the palaeontological panacea for students in schools, colleges and universities; a claim which is not made in the preface and which cannot be substantiated. Thus the author's intentions have resulted in a beautifully illustrated palaeontological textbook of the classical British school. As that it could be welcomed and praised for its breadth of scope and general high quality of its numerous photographs.

It is particularly useful in that the usual range of creatures is illustrated along with photographs of diatoms, antiarchs, coccoliths, bryozoan zoecia, and the alga *Solenopora*, all of which are usually ignored at this level. The layout is orderly. The invertebrate phyla are systematically treated under headings of morphology, mode of life, and geological history. A short technical description of characteristic British genera and their stratigraphical range is given and the chapters are closed with a list of technical terms. The index is reliable.

But on close inspection the labelling of diagrams leaves something to be desired, for example, throughout figure 130 on graptolite structures, the plural form is used yet only one theca is marked in each instance; the diagrams of the corals are particularly disappointing. The book's weaknesses lie in its treatment of the biological and evolutionary aspects, preservation, and the extremely limited suggestions for further reading and reference.

JULIA A. E. B. HUBBARD

Problems in Childhood

A Neuropsychiatric Study in Childhood. By Michael Rutter, Philip Graham and William Yule. (Clinics in Development Medicine Nos. 35/36.) Pp. vi+272. (Spastics International Medical in association with Heinemann Medical: London; Lippincott: Philadelphia, 1970.) £3.75.

THERE are few trickier problems than that presented by the child with a behaviour disorder—and behaviour disorders are very common in children. They range from the serious conditions associated with mental retardation or epilepsy to many milder disorders, such as bed-wetting and anti-social behaviour at school. The neurologist may diagnose "minimal brain damage" while the psychiatrist thinks in terms of early maternal deprivation. The viewpoints of parent and teacher are different again. The authors of this book are well known as leading investigators in this field and they have performed a valuable service in producing an eminently practical book, clearly written in chiefly non-technical language and giving a sensibly balanced view. The book is well produced, well illustrated, and it maintains a high standard throughout. It can be recommended warmly to all who are looking for practical guidance in the subject.

DEREK RICHTER

Control of Malaria

Chemotherapy and Drug Resistance in Malaria. By W. Peters. Pp. xvi+876. (Academic: London and New York, December 1970.) £13.00.

TWENTY years ago the appearance of anopheline mosquitoes resistant to residual insecticides spurred the World Health Organization into a programme designed to eradicate malaria from the world. Resistance of vector parasite to drugs seemed to pose no threat to the success of the campaign. Today, most vector mosquitoes remain susceptible to DDT.

It would be an exaggeration to say that drug resistance in malarial parasites has been the principal cause of slow progress towards the eradication of malaria, but the increasing insusceptibility of *Plasmodium falciparum* to chloroquine in South America and South-East Asia is serious—both for malariologists faced with the prospect of foci of infection due to resistant parasites, and for the clinician who may find that half his patients suffering from falciparum malaria are not cured by the standard treatment with chloroquine.

Professor Peters's book could not have appeared at a more opportune

moment though topicality has disadvantages. Many of us require up to date information on this subject, but changes are occurring so rapidly that any review aiming at completeness soon becomes obsolete. Originally, the book set out to review all important work on the subject up to the end of 1968; but, to deal with later work up to the autumn of 1969, an addendum of 36 pages reviews 240 more recent papers. This arrangement, though demanding constant reference to the addendum, succeeds in keeping the book up to date. Peters assures us that the tempo of malaria research now shows signs of slackening; should one be ashamed to admit a feeling of relief?

Two factors combine to make this book stand out above similar reviews of subjects of current scientific importance. The first is Peters's own career. For many years he was a "wet-foot" malariologist in some of the most difficult countries in the world, including Nepal, Liberia and New Guinea. During the last ten years his work has been in biological laboratories in Switzerland and Liverpool. There can be few scientists who can write with such authority, both on basic research and on its application in the control of a disease that afflicts "poor people in obscure places".

The second factor that makes this work outstanding is the quality of the language. There are a few review books of this type that are a pleasure to read. Davidson and Passmore's *Human Nutrition and Dietetics* is one; Peters has produced another. The clarity of the language makes the matter digestible and the frequent happy phrasing adds a spice of enjoyment. This is particularly true of the introduction, which summarizes the aims of the book and gives the historical background of chloroquine resistance.

The early chapters deal with basic information about the malaria parasite, the host-parasite relationship and the techniques employed in evaluating drug response both in man and animals. The principal part of the book discusses systematically, drug by drug, experimental resistance, both in the laboratory and its practical application in human malarias. Special aspects of drug resistance, described separately, include the influence of drug combinations, entomological, immunological and genetical aspects. The final chapters present the practical effects of drug resistance in the field.

The references and subject indices, essential in a book of this nature, are of a high standard. For the field worker a place index would have been useful to give easy access to information about resistance in the country with which he is concerned. It is not

easy to find this information quickly in the subject index. The only other criticism is of the title itself. This is a definitive work on drug resistance rather than on the chemotherapy of malaria. But these are minor points to make about such an excellent book.

It covers such a wide field that the impression of the reader will tend to reflect his own interests. I was particularly interested in the story of the downfall of "infallible" chloroquine as a weapon against *P. falciparum*, the critical examination of methods of evaluating this failure in the field and in methods of preventing or eliminating drug resistance. The absence, so far, of chloroquine resistance in Africa is of vital interest. This could be due to less intense use of the drug than in South-East Asia or South America, but Peters holds out some hope of other explanations.

Peters gives several examples of apparent drug failure being really due to failure of the patient to swallow the tablets. This practical difficulty was probably the principal cause of the failure of the drug trial at Kankiya, which had convincing mathematical arguments in its favour.

The vital problem of the place of drugs in the future of malaria control emphasizes the value of Peters's experience. New drugs are required and must be evaluated in the laboratory, but it requires a malariologist to assess their practicability as a control measure in the field. Both aspects are combined in this splendid book.

M. J. COLBOURNE

Wasp Biology

The Wasps. By Howard E. Evans and Mary Jane West Eberhard. Pp. vi+265. (University of Michigan: Ann Arbor, November 1970.) \$3.45 paper; \$7.95 boards.

THIS book embraces a very wide and richly varied field of wasp biology. An introductory chapter briefly describes the principal groups of wasps, their probable evolution, their representation in North America and their importance to man. Whereas some wasp species are regarded as pests because of their habit of raiding honeybee colonies, others have been used in the biological control of plant pests and some species of social wasp in Brazil and Mexico store sufficient nectar to make exploitation by man worth while.

Chapter 2, on the natural history of wasps, is the longest chapter and is in many ways one of the most interesting. It gives a comparative account, together with the underlying evolutionary themes, of a number of biological topics that are common to wasp species.

These topics include the egg stage, larval and pupal stages, adult feeding and food, predatory behaviour, prey carriage and orientation to the nest. The amazingly diverse nests and types of nesting behaviour of solitary wasps are the subjects of Chapter 3. Many of the examples of nesting behaviour that are quoted are of great interest to students of animal behaviour; for example, several species of *Ammophila* use a pebble as a tool to hammer down the material filling the entrance to a burrow, and certain species of *Sphex*, *Bembix* and *Philanthus* dig small holes or accessory burrows which divert the attention of parasites away from the real entrance to the nest and which are reminiscent of the false entrances of Neolithic burial mounds. In fact, the contribution that wasp studies have made to the field of animal behaviour is quite remarkable. Nest building behaviour, prey collection, nest provisioning and nest site learning provide classical illustrations of sequential behaviour patterns, performed in the presence of appropriate releasers, which often exhibit a certain degree of flexibility. These and many other fascinating subjects, including pseudo-copulation with orchids which mimic female wasps, are lucidly and enthusiastically portrayed.

The next two chapters are concerned with the social wasps, their colony initiation, growth and termination, nest architecture, foraging, brood care, division of labour and communication between individuals within a nest, caste determination, mating and hibernation. Finally, Chapter 7 describes many of the wasps, flies and beetles that are parasitic on wasps and their nests, and finishes with an evaluation of the survival value of wasps' aposematic coloration.

The evolutionary backbone running through this book reaches its climax with a discussion of the origins of social life in the Hymenoptera. Because ants and bees are believed to have come from wasp-like ancestors, the wasps may hold the key to this interesting problem. The principal steps in the evolution of wasps' nesting behaviour are traced, ending with communal living, prey maceration and adults feeding larvae other than their own. The recently propounded theory on the part which the haploid-diploid mode of sex determination has played in facilitating sociality among the Hymenoptera receives special treatment.

It is, of course, possible and profitable to compare the behaviour of wasps with that of other Hymenoptera, and the parallels between wasp and bee behaviour and the associated problems are often particularly striking. The current trend in the use of trap nests to investigate solitary wasps and bees

has focused interest on the common problems of development and emergence of insects produced in a linear series of cells. Indeed, even the correct orientation in their cells of the pupae of both solitary wasps and honeybees seems to depend upon similar sensory cues. Solitary bees are renowned for their specific foraging preferences and most solitary wasps also specialize in a particular kind of prey, but, in general, social wasps, like social bees, have lost their host specificity, no doubt because they are not limited to one brood generation and a brief foraging period.

The direct transfer of food between the members of a social insect colony was first demonstrated for wasps, and the realization of its extensiveness and importance in ants, bees and termites as well as wasps has allowed many important advances in our understanding of insect societies to be made. When a wasp larva receives an appropriate signal from an adult it exudes a drop of fluid from its mouth which the adult imbibes. Although this behaviour (trophallaxis) has long been known, the value, if any, of the fluid to the adult is still a matter of controversy. Indeed, as the authors point out, despite the large amount of current research on wasps and other social insect colonies we still do not know precisely how the colonies are organized.

There is a suggested reading list of 10 publications. Although 40 authors are mentioned in the text the sources of their work are not given, but nevertheless the authors are to be congratulated on bringing together such a large amount of fascinating information and presenting it in an eminently readable manner.

JOHN B. FREE

Sounds Submerged

Underwater Acoustics. By Leon Camp. Pp. xi+308. (Wiley-Interscience: New York and London, November 1970.) £8.25.

THE origin of this book was an intensive sixty hour lecture course at the University of California given to practising engineers with various academic backgrounds. Because the underwater acoustics field is very broad, the authors admit they had difficulty selecting material so as to keep the book to a manageable size. On this point, the authors are to be congratulated. Their coverage is quite adequate for either final year undergraduates or graduate engineers just entering the field, for whom the book is presumably intended.

Introductory chapters deal with the mechanics of vibration, and introduce

the ideas of complex number notation, phase, resonance and selectivity. Wave and ray acoustics are described in detail, together with various types of transducers and array radiation patterns. The analysis of some simple sonar situations and the use of matched filter and signal processing techniques conclude the text.

Unfortunately there are many errors in the book, and while many of these are easily spotted (equations dimensionally incorrect), readers may find the mistakes irritating and so lose confidence in the authors' integrity. It is sad that most of the errors are due to poor editing and careless proof reading.

For a book meant for students new to the underwater acoustics field it fails to present the material in a readable manner. Emphasis is given to mathematical dexterity, without explaining the meaning of many equations. For example, when the circular locus of a particular ray path is derived, the radius of curvature is given as $-Co/g$, where Co is a velocity and g a velocity gradient. There is no explanation of why the radius of curvature appears to be negative and inconsistencies in an associated diagram make the situation even more confusing.

Underwater acoustics involves the disciplines of acoustics, mechanics, and electricity, and notation conventionally used in one of these disciplines often has a completely different meaning in the others. The authors have used the notation applicable to the discipline being discussed at a particular point, and this can be very confusing, especially as no list of symbols is available except for those pertaining to one chapter. Thus it is impossible to use the text as a reference book, for time has to be spent searching for the meaning of symbols.

The last two chapters on sonar systems and signal processing include material fresh to most texts on the subject. Apart from these chapters, the book adds nothing to the range of similar books currently available.

D. J. CREASEY

Luminescent Molecules

Fluorescence Analysis: a Practical Approach. By Charles E. White and Robert J. Argauer. Pp. x+389. (Dekker: New York, September 1970.) \$18.75; £8.90.

MOST molecules absorb light somewhere in the ultraviolet/visible/infrared region of the electromagnetic spectrum and therefore absorption spectrophotometry in solution is probably the most used analytical technique for the determination of a great number

of substances. Absorption bands are broad, however, and chemically similar substances have overlapping spectra, which makes the determination of one substance in the presence of others extremely difficult. Nevertheless, very small differences in molecules which scarcely affect the absorption spectrum can cause quite profound changes in the stability of the electronic state to which the absorbing molecule is promoted by absorption radiation. These molecules which have stable excited states are usually capable of some form of luminescence. Because only a few molecules fluoresce, the selectivity of fluorescence analysis is usually much superior to absorption analysis. In addition, fluorescence analysis is almost invariably more sensitive for reasons which need not be discussed here.

Fluorescence analysis has, however, for inexplicable reasons never achieved much popularity in the Western world, though other countries, particularly the USSR, seem to have used it to good effect much more extensively. More recently the twin problems of "source scatter" and "quenching", which quite unrealistically seem to have turned so many potential users against the fluorescence technique, have been more fully appreciated as relatively minor in importance, and increasing use is now being made of this method of analysis.

This book by White and Argauer gives a fairly full and detailed account of the practice of fluorescence analysis in a modern laboratory. The authors point out that there are many theoretical treatments of fluorescence phenomena, and after citing some of these they get down to their subject with commendable vigour. Instrumentation is reviewed in a chapter which may unfortunately become dated rather quickly if the topic continues to grow at its present speed, and this is followed by a very fine exposition of the correction of excitation and emission spectra—one of the best accounts of this topic that I have seen. Subsequent monographic chapters deal with fluorescent metal chelates, quantitative methods, fluorescent indicators and the application of spectrofluorimetry to separation techniques such as chromatography and to agricultural problems. Special contributions are then made on the applications of fluorescence to the study of proteins and amino-acids, polynuclear aromatic hydrocarbons and other organics and, in particular, vitamins and steroids. The phenomena of chemiluminescence, X-ray fluorescence and atomic fluorescence (wrongly labelled as "resonance" in the latter instance) are discussed usefully, and finally there is a good account of clinical fluorimetric procedure.

T. S. WEST

CORRESPONDENCE

Responsibility in Science

SIR,—*Nature's* editorial (229, 513; 1971) on BSSRS, the British Association, and the problems of science and society ignores one of the crucial differences of approach between the two organizations.

Nature concedes that there is "no harm" in examining the social problems brought about by science, and adds that "there is even a need that public attention should be drawn to some of these issues". This demands, however, more than the occasional attention of a few "sensible" and "sober" individuals. We in BSSRS are concerned with analysing the values of modern science, the relationships of science with society, and the ways in which these relationships can, where necessary, be changed. And

unlike *Nature*, or, apparently, the British Association's council, we do not feel it possible or desirable to divorce the ethical problems of science from the processes of "radicalization of science".

We do not see our role, as *Nature* seems to, as that of an "enlightened pressure group". To present packaged solutions to complex social problems is the type of élitist decision-making we are trying to avoid; instead of conventional minority pressure group politics, we are concerned to create a climate in which socially irresponsible decisions are confronted by an informed and active public opinion.

We offer proposals for policy where these are relevant, and have already done so over CS, herbicides, sponsored research in universities, and the dumping

of radioactive waste. But this is secondary to the main task, that of analysis and criticism through which we can mobilize the social conscience both of the scientific community and of society in general.

Yours faithfully,

PETER CHAPMAN
DAVID DICKSON
SHIVAJI LAL
JERRY RAVETZ
HILARY ROSE
STEVEN ROSE
JONATHAN ROSENHEAD
DAVID WIELD
MAURICE WILKINS

*British Society for Social
Responsibility in Science,
70 Great Russell Street, London WC1*

Announcements

Miscellaneous

THE inaugural meeting of the Lysosome Club was held on January 23, 1971, and Dr A. Allison, Dr J. Dingle, Professor T. Slater, Dr L. Bitensky and Dr S. F. Contractor were elected to the committee for 1971.

The aims of the Club are to bring together informally scientists working in the field two or three times a year, with one meeting in London and others in the provinces. Any scientist working in this field who would like to join in the

activities of the Club should write to the honorary secretary, Dr S. F. Contractor, Charing Cross Hospital Medical School, 62 Chandos Place, London, WC2, enclosing the membership fee of £0.50.

ERRATUM. Due to an administrative error the penultimate paragraph was unfortunately omitted from Professor W. S. Bullough's article "The Ageing of Mammals" last week (*Nature*, 229, 608; 1971). The paragraph was as follows:

"As Comfort has pointed out, the key to the problem of prolonging the active life of man lies in the understanding of the

basic mechanism of the ageing process, and the advantage of the present theory is that it is immediately open to an experimental approach. A technique now exists for manipulating the duration of mammalian life in a reproducible manner using only normal physiological means. This opens the way for a study of the causes and consequences of the prolongation of life in laboratory mammals and ultimately in man; it has been the lack of such a technique that has for so long prevented any rational study of the biology of mammalian ageing. It is also obvious that even if the present theory should prove to be wrong, the exploitation of this technique may enable the truth to be reached."

British Diary

Monday, March 8

Communication of Objectives—Reconciling the Interests of the Organization and the Engineer (5.30 p.m.) Dr D. Pym, Institution of Electrical Engineers, at Savoy Place, London WC2.

"Ford Commuter"—Electric Urban Vehicle (6.30 p.m.) Mr T. K. L. Dobedoe and Mr R. F. Robbins, Institution of Electrical Engineers, London Graduate and Student Section, at Thames Polytechnic, Wellington Street, London SE18.

Recent Progress on Semiconductor Microwave Sources (2 p.m. colloquium) Institution of Electrical Engineers, at Savoy Place, London WC2.

Tuesday, March 9

Air Conditioning for Human Comfort (7.30 p.m.) Mr J. Bruce, Society of Environmental Engineers, in the Mechanical Engineering Department, The University, Birmingham.

Carbon Black in Coating Application—Dispersion and Formulation Techniques (7.30 p.m.) Mr L. A. Tysall and Dr D. H. Shearer, Oil and Colour Chemists' Association, at the Griffin Hotel, Boar Lane, Leeds.

Guided Electromagnetic Waves (6.30 p.m.) Professor H. M. Barlow, FRS, Institution of Electronic and Radio Engineers, at University College London, Gower Street, London WC1. (Seventh Clerk Maxwell Memorial Lecture.)

Regulation of Enzyme Synthesis by Phytochrome (5.30 p.m.) Professor H. Mohr, University of London, in the Anatomy Theatre, University College London, Gower Street, London WC1.

Richard Owen, First Director of the Museum (6.30 p.m.) Dr M. Rudwick, British Museum (Natural History) and the Victorian Society, in the Lecture Hall, British Museum (Natural History), Cromwell Road, London SW7.

The Anatomy of the Organ of Voice in Some Genera of Anatidae (Aves: Anseriformes), Dr R. W. Warner;

Wild World, Dr J. H. Sparks (special film on deer); **The Seals of Macquarie Island** (film, CSIRO Australia) (5 p.m.) Zoological Society of London, at the Zoological Gardens, Regent's Park, London NW1.

The Flow of Information in a University (1.20 p.m.) Professor H. Kalmus, University of London, in the Botany Theatre, University College London, Gower Street, London WC1.

Wednesday, March 10

Aspects of Military Defence Communications: Past and Future (5.30 p.m.) Mr J. R. Mills, Institution of Electrical Engineers, at Savoy Place, London WC2.

Chemical Microscopy—The Overlooked Technique (2.30 p.m.) Society for Analytical Chemistry, at the University, Birmingham.

Modernization of Shortwave Transmitting Stations (6 p.m.) Mr C. MacKenzie, Institution of Electronic and Radio Engineers, at 9 Bedford Square, London WC1.

Starting Methods for Reversible Generator/Motor Units Employed in Pumped Storage Stations; and An Investigation into Damping Torque Coefficients and Stability of a Two Synchronous Machine System Employed in Synchronous Starting (5.30 p.m.) Dr T. J. Hammons and Mr J. Loughran, Institution of Electrical Engineers, at Savoy Place, London WC2.

Stereophonic Broadcasting (7 p.m.) Dr G. J. Phillips, Institution of Electrical Engineers, at the SEB, 1 Woodstock Road, Yarnton, Oxford.

The Origin and Publication of some Important Scientific Books and Papers (1 p.m. general discussion) Royal Institution, History of Science Discussion Group, at 21 Albemarle Street, London W1.

The Problem of Differentiation: A New Approach (5.30 p.m.) Professor H. Mohr, University of London, in the Botany Theatre, University College London, Gower Street, London WC1.

The Victoria Line (6.30 p.m.) Mr V. H. Smith, Institution of Electronic and Radio Engineers, at the Central Library, Romford, Essex.

The Work of the Alkali Inspector (8 p.m.) Mr E. T. J. Fuge, Society for Analytical Chemistry, jointly with the RIC, at Teesside Polytechnic, Borough Road, Middlesbrough.

Thursday, March 11

Electromagnetic Waves. X-ray Diffraction (1 p.m. films) Royal Institution, at 21 Albemarle Street, London W1.

Inorganic Pigments (7 p.m.) Mr R. W. Batchelor, Colour Group (Great Britain), at the North Staffordshire Polytechnic, Stoke-on-Trent, Staffordshire.

Linear and Incremental Actuators (5.30 p.m. discussion) Institution of Electrical Engineers, jointly with the I.Mech.E., at Savoy Place, London WC2.

Stereo Transmission (7.15 p.m.) Dr G. J. Phillips, Institution of Electrical Engineers, at Aylesbury College of Technology, Aylesbury, Buckinghamshire.

Notes to Authors

Will contributors to *Nature* follow as far as possible the general style of the journal, and in particular the following rules:

1. Manuscripts should be typed in double spacing and submitted in duplicate. Two copies of all figures should be included, one for the printers and one for the referee.

2. References are indicated by superscript numbers in the order in which they appear in the text, and have the following form:

¹ Sargent, W. L. W., and Searle, L., *Astrophys. J. Lett.*, **162**, L155 (1970).

² Fraenkel-Conrat, H., in *Comprehensive Biochemistry* (edit. by Florkin, M., and Stotz, E. H.), **7**, 75 (Elsevier, Amsterdam, 1963).

"Unpublished work", "Private communication" and "Work in preparation" should not be cited as references but may be incorporated in the text. Each reference number should refer to an individual work which has either been published or is in the press.

3. Although the text of longer articles is usually interrupted by headings at suitable intervals, these should be short and informative and not merely "Introduction", "Results", or "Conclusion".

4. Units should either conform to the SI system or be spelled out in full.

A fuller guide can be had from the *Nature* offices in London and Washington.

The Determination of Prophylactics in Feeding Stuffs (6.30 p.m.) Mr R. E. Weston, Society for Analytical Chemistry, at Liverpool Polytechnic, Byrom Street, Liverpool.

Superplasticity (7 p.m.) Professor R. B. Nicholson, Liverpool Metallurgical Society, in the Department of Metallurgy and Materials Science, The University, Liverpool.

The Anti Scientific Revolution (7.30 p.m.) Mr John Wren-Lewis, H. G. Wells Memorial Lecture, in the Old Chemistry Department, Imperial College, Imperial Institute Road, London SW7.

Friday, March 12

Application of Time-resolved Mass Spectrometry to Gas Phase Flash Photolysis (1 p.m.) Dr D. Price, Royal Institution, Photochemistry Discussion Group, at 21 Albemarle Street, London W1.

Concorde (9 p.m.) Sir George Edwards, FRS, Royal Institution, at 21 Albemarle Street, London W1.

Molecular Sieve Chromatography (2.30 p.m.) Mr D. M. W. Anderson, Society for Analytical Chemistry, Chromato-

graphy and Electrophoresis Group, at Derby and District College of Technology, Derby.

Review of Transformer Noise Suppression and Vibration Isolation (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

The Interior of the Earth and Moon (4.30 p.m.) Professor Frank Press (Harold Jeffreys Lecturer), Royal Astronomical Society, at the Scientific Societies Lecture Theatre, 23 Savile Row, London W1.

Saturday, March 13

Expedition Iceland (3.30 p.m.) Mr David Stanbury, Inner London Education Authority, at the Horniman Museum, London Road, Forest Hill, London SE23.

Monday, March 15

A Behaviourally Testable Theory for Belief and Preference (5.30 p.m.) Professor R. B. Braithwaite, British Society for the Philosophy of Science, in the Joint Staff Common Room, University College London, Gower Street, London WC1.

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